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THE

EDINBURGH
MEDICAL AND SURGICAL
JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND
PHARMACY.

VOLUME TWENTY-SECOND.

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Communications have been received from Dr Campbell, Lancaster; Dr F. Cobb, London; Dr Darwall, Birmingham; Dr O'Donovan, Bantry; Dr M. Sinclair, Edinburgh; Mr Braid, Leadhills; Mr Neilson, Glasgow; Mr Sym, Kilmarnock.

The following Publications have been received—

Observations on Injuries of the Spine and of the Thigh-bone: in two Lectures, delivered in the School of Great Windmill Street. The first in Vindication of the Author's Opinions against the Remarks of Sir Astley Cooper, Bart. The second on the late Mr John Bell's Title to certain Doctrines now advanced by the same Gentleman. Illustrated with nine Plates. By Charles Bell, Surgeon to the Middlesex Hospital. 4to. pp. 101. London, 1824.

An Account of a Case of Recovery, after the Shaft of a Chaise had been forced through the Thorax: To which is now added, A Statement of the Health of the Sufferer, from the period of his Recovery, until his Decease, with the Appearances of the injured parts after death. By William Maiden, Member of the Royal College of Surgeons in London. 4to. pp. 59. London, 1824.

On the Nature of the Acid and Saline Matters usually existing in the Stomachs of Animals. By William Prout, M. D. F. R. S. (From the Philosophical Transactions.) 4to. pp. 7. London, 1824.

An Elementary System of Physiology. By John Bostock, M. D. F. R. S. L. S. and H. S. M. R. I., &c. &c. Vol. I. 8vo. pp. 518. London, 1824.

Principles of Medical Science and Practice. Part I. Physiology. By Hardwicke Shute, M. D. Physician to the General Infirmary, and to the County and City Lunatic Asylum, Gloucester. 8vo. pp. 565. London, 1824.

Medical and Surgical Cases; selected during a Practice of thirty-eight years. By Edward Sutleffe, Queen Street, London. 8vo. pp. 628. London, 1824.

A Practical Treatise on Diseases of the Skin, comprehending an Account of such Facts as have been recorded on these subjects, with original Observations. The whole arranged with a view to illustrate the constitutional Causes of these Diseases, as well as their local characters. By Samuel Plumbe, Member of the Royal College of Surgeons of London, the Medico-Chururgical Society, &c. &c. 8vo. pp. 392. London, 1824.

Original Cases, with Dissections and Observations, illustrating the use of the Stethoscope and Percussion in the Diagnosis of Diseases of the Chest; also Commentaries on the same Subjects selected and translated from Auenbrugger, Corvisart, Laennec and others. By John Forbes, M. D. Physician to the Chichester Dispensary. 8vo. pp. 304. London, 1824.

Practical Remarks. Part I. On Acute and Chronic Ophthalmia, Ulcers of the Eye, &c. &c. Part II. On Remittent Fever, viz. Simple and Complicated. By Thomas O'Halloran, M. D. Author of Treatises on Barcelona and Andalusian Yellow Fever Epidemics. 8vo. pp. 148. London, 1824.

Practical Remarks on Lacerations of the Uterus and Vagina, with Cases. By Thomas M'Keever, M. D. late Assistant to the Dublin Lying-in Hospital. 8vo. pp. 82. London, 1824.

Practical Observations on Hydrophobia, with a Review of the Remedies employed, and Suggestions for a different Treatment of that Disease. By John Booth, M. D. formerly a President of the Royal Medical Society of Edinburgh, Member of the Royal College of Physicians of London, and one of the Physicians to the Birmingham General Hospital. 8vo. pp. 16. London, 1824.

A System of Anatomical Plates; accompanied with Descriptions, and Physiological, Pathological and Surgical Observations. By John Lizars, F. R. S. Fellow of the Royal College of Surgeons, and Lecturer on Anatomy and Physiology, Edinburgh. Part IV. The Muscles of the Trunk. 8vo. pp. 43. Edinburgh, 1824.

Part V. of the same Work, containing Muscles and Joints of the Upper and Lower Extremities. This Part is accompanied with two Supplemental Plates to Part IV. illustrative of Hernia, from Dissections by the Hand of Sir Astley Cooper, Bart., Surgeon to the King, kindly presented to the Author, and preserved in his Museum at Edinburgh. 8vo. pp. 137. Edinburgh, 1824.

The Medico-Chirurgical Review, and Journal of Medical Science (Quarterly) No. 2. New Series for September 1824.

Surgical Essays by Baron D. J. Larrey, Surgeon in Chief of the Royal Guard, &c. &c. Translated from the French by John Revere, M. D. &c. &c. 8vo. pp. 335. Baltimore, 1823.

A Summary of Physiology. By F. Magendie, Doctor of Medicine of the Faculty of Paris, Professor of Anatomy, &c. &c. Translated from the French by John Revere, M. D. &c. &c. Second Edition. 8vo. pp. 444. Baltimore, 1824.

Essays on various Subjects of Medical Science. By David Hosack, M. D. F. R. S. L. & E. &c. &c. 2 Vols. 8vo. pp. 380. New-York, 1824.

A Fasciculus, containing nine Lithographic Anatomical Drawings, from Preparations in the Museum of the Army Medical Department at Chatham. London, 1824. Large Folio.

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4. An Historical and Practical Treatise on the Internal Use of the Hydrocyanic (Prussic) Acid in Pulmonary Consumption, and other Diseases of the Chest, as well as in several Complaints, &c. &c. &c. Illustrated by numerous Cases. By A. B. Granville, M.D. F.R.S. F.L.S. M.R.I.
5. C. W. Hufeland, Vorschlag, statt der Blausäure das destillirte Wasser der bittern Mandeln zum medizinischen Gebrauch anzuwenden, &c. &c.
6. Observations on the Remarkable Effects of Iodine in Bronchocele and Scrophula. Being a Translation of Three Memoirs published by I. R. Coindet, M. D.
7. Saggio Clinico Sull' Iodio e sulle differenti sue Combinazioni Farmaceutiche giusta i risultamenti che se ne sono ottenuti nell'istituto Clinico-Medico dell' I. R. Universitá di Padova. Estratto dal Prospetto Clinico dell' Anno Scolastico 1820–1821.
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EXPLANATION OF THE PLATE.

The side figure represents a section of the biliary concretion described by Dr Craigie.

Fig. 1. Illustrates Note to page 298.
2. Is explained at page 299.
3. F I, Fascia iliaca. F L, Fascia lata. P, Section of *symphysis pubis.* V, Femoral vessels passing from the pelvis into the thigh. See p. 302.
4. Shows the relative situation in which the fasciæ would appear, if a perpendicular longitudinal section were made at the part pointed out by (*a*) in Fig. VII. See p. 303.
5. Shows the relative situation in which the fasciæ would appear, if a section was made at (*b*) Fig. VII. See page 304.
6. Plan of the crescentic portion of the crural arch, as seen from before. F L, Fascia lata. F T ^{s}, Fascia transversalis. P L, Peupart's ligament; in other words, the inverted margin of the fascia tendinosa. V, the vein. A, the artery. S, part of the attachment which constitutes the stricture in femoral hernia. F P, Falciform process of fascia lata. N. B. The operation is performed best by cutting either at S, or at F P.
7. Superficial view of the groin, to explain the points at which I wish the imaginary sections to be made. See pages 303 and 304.

Note to Fig. 2.

The reader may, perhaps, remark a striking, I may almost say, precise resemblance between this figure and one given by M. Cloquet in a late Number of his excellent Anatomical Plates. It gives me no small pleasure to find that my ideas on this subject agree with those of an anatomist so celebrated. And I may add, that the drawing represented here, was executed, together with the others, by my friend and late pupil Mr Evans, many months ago, while I was entirely ignorant of Cloquet's figure, which I never saw or heard of, until on my return from Germany a few weeks ago, I examined the contents of a box of books which had arrived from Paris in my absence.

J. S.

THE
EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

1. JULY, 1824.

PART I.

ORIGINAL COMMUNICATIONS.

I.

CONTRIBUTIONS TO THE PATHOLOGY OF THE STOMACH, THE PANCREAS, AND THE SPLEEN.

By JOHN ABERCROMBIE, M.D., FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH.

PART IV.

DISEASES OF THE SPLEEN.

THE functions of the spleen are still involved in great obscurity; but the pathology of it presents some interesting subjects of investigation. Though it cannot perhaps be considered as a vital organ, it appears to exert a most important influence upon the functions of the stomach; and many facts connected with its diseases, show, in a striking manner, the importance of this organ in the animal economy.

The spleen appears to be liable to inflammation both acute and chronic, and to suppuration, and other consequences of inflammation; but the symptoms accompanying these affections do not appear to have been sufficiently investigated. It is also liable to tubercular disease, and the slow suppuration which usually supervenes upon that affection. I had lately an opportunity of seeing the spleen studded throughout with innumerable tubercles, all in the solid state, in the body of an infant aged eight months, who died of extensive disease of the bronchial glands. In a more advanced stage of this affection, it has been found to present numerous small abscesses, like the small vomicæ of tubercular lungs. The disease, however, is usually complicated with tubercular disease in other organs, so that it is impossible to ascertain the symptoms which arise from the affection of the spleen.

The spleen, in its structure, does not strictly resemble any other organ in the body, and therefore it may be doubted whether, in judging of the effects of inflammation on the spleen, we are to be entirely guided by the effects which we know to arise from it in other organs. There is particularly one appearance which I suspect arises from inflammatory action, and which seems to be almost peculiar to the spleen. It consists of the whole substance of it being reduced to a soft mass of a dark colour, in some cases resembling a mass of coagulated venous blood, and breaking down under the slightest pressure, after its peritoneal coat is laid open; in others being still softer, of a pultaceous consistence, or nearly fluid. This condition of the spleen I have observed as the only morbid appearance in some obscure cases, which were fatal, with symptoms referable to the stomach. The following observations will illustrate the affection, which seems to present a subject of some interest.

CASE XV.—A lady, aged 60, had been for several months affected with loss of appetite, dyspeptic symptoms, and occasional vomiting. I attended her for about a month before her death, during which time she had daily vomiting; generally four, five, or six times a day; little or no appetite; tongue loaded; bowels rather costive, but easily kept open; pulse natural. She did not complain of any pain, and nothing could be felt, on pressure, that could account for the symptoms. With the symptoms now mentioned, and frequent nausea, she sunk gradually, and died exhausted in about a month. *On dissection*, no morbid appearance could be discovered after the most careful examination, except in the spleen, which was of a very dark colour, and remarkably soft, and broken down like grumous blood.

In Dr Johnston's Medico-Chirurgical Journal, Vol. III., is mentioned the case of a lady, aged 41, who after having been for some time affected with nausea and loss of appetite, had severe nausea and frequent vomiting for about a fortnight—the pulse quite natural. The symptoms then subsided considerably, and she was considered as getting better, when she died suddenly after a violent fit of vomiting, in which she had been much exhausted. On dissection, there was observed some slight appearance of inflammation on the lower intestines, and the spleen was very soft and broken down into a mass like coagulated blood: The bowels had been quite natural.

A man mentioned by Sennertus, had been affected for some weeks with loss of appetite and pain in the left side. He was then seized with discharge of blood by stool, and died in 15 days. On dissection, the pancreas was found slightly diseased, some parts of it being indurated, and others soft; but the principal morbid appearance was in the spleen, which was entirely resolved into a bag, full of a matter like the lees of oil (*amurca*), and somewhat fetid: No part of the natural substance remained. Many other cases are on record, in which this appearance of the spleen, under various modifications, was the prominent morbid appearance. Some authors compare it to a bag of very fetid pitch, others call it putrefaction, and others gangrene. The symptoms also varied considerably, in some cases being rather chronic and obscure, as in the cases now mentioned; in others being more acute, and the progress more rapid. A woman aged 33, mentioned by Grotanelli, was seized with fever, headach, rheumatic pains, and a deep-seated pain in the left side. On the 7th day, she became delirious and comatose, and died on the 9th. Some effusion was found in the brain; the liver was somewhat enlarged, and the spleen was reduced to a bag containing a soft black pulp. A man mentioned by Lossius, had violent pain in the left hypochondrium, extending to the shoulder with acute fever; and one by Bonetus had violent pain of the stomach with black vomiting. In the case by Lossius, the appearance was combined with marks of inflammation in the neighbouring parts; and in two cases by Crendal, it was found in connexion with extensive peripneumonia. I have also observed it in one or two cases in which there had been extensive inflammation of the lower part of the left lung. Upon the whole, there is much reason to believe that this condition of the spleen is a fatal disease; and there are several circumstances which render it probable that it is the result of inflammatory action. We have other examples of morbid conditions, differing from the ordinary effects of inflammation, but which appear to be the result of it in certain parts, in connexion with peculiarities in their structure. The most remarkable is the pultaceous softening of the brain, which the French writers have described as a distinct and peculiar disease, but which there seems every reason to consider as a result of inflammation.

The spleen is also liable to *suppuration*, which may go on with very obscure symptoms, as in the following remarkable case.

Case XVI.—A gentleman, aged 52, who had previously enjoyed very good health, was, in January 1821, affected with slight feverishness, like a common cold. After a short confinement, he was considerably better, and went out; but after a short time was confined again, though without any defined complaint, except a feeling of weakness. At the commencement of his illness there had been slight cough, but this disappeared entirely at an early period. When closely questioned, he mentioned occasionally an undefined uneasiness across the epigastric region, but it was slight and transient, and he never complained of any other pain. His appetite was variable and capricious, but upon the whole not bad, and he had no dyspeptic symptom; his bowels were rather slow but easily kept open; his breathing was natural, and every other function was in the healthy state, except that his pulse continued a little frequent; and he was becoming progressively more and more weak and emaciated. In this manner the complaint went on during the remainder of the winter. In the beginning of summer he went to the country, where, however, he made no improvement. He was now greatly reduced in flesh and strength; his pulse usually from 96 to 100, and weak; his nights were generally good, but sometimes feverish; his appetite was bad, but he still took a good deal of nourishment, and never complained of his stomach; there was no cough and no pain; the urinary secretion and bowels were natural; but the debility and emaciation continued to increase gradually; and he died on the 5th of July, having been seized with diarrhœa three days before his death. Before the attack of diarrhœa, there had been little change for some weeks; he had been able to be out of bed the greater part of the day, and occasionally out in a carriage or a garden chair. *Dissection.*—The spleen was somewhat enlarged, and in the centre of it there was an irregular cavity, containing several ounces of purulent matter. The surrounding substance was soft and easily lacerated. The liver was pale, but otherwise healthy. The kidneys were pale, with a peculiar degeneration of some parts of them into a white substance, in its texture retaining the firmness of the other parts, but in its appearance resembling the substance of the brain. After the most careful examination, no other morbid appearance could be detected in any of the viscera.

From the commencement of his illness, this gentleman was under the able management of Mr William Wood, and in the progress of it he was occasionally seen by Dr Thomson and myself: but we never could detect a symptom from which we could ascertain what was the seat of the disease. Towards the conclusion of his life, a circumstance occurred which deserves to be mentioned, though not connected with the fatal disease. A few weeks before his death, while one day turning his right arm suddenly behind his back, he felt an acute pain a little below the bend of the elbow. The part was a little hard and painful to the touch, and no pulsation could be felt in the arteries below it, on the fore-arm. In the humeral artery, the pulsation was natural. After a few days, the pulse was again felt on the fore-arm; but, after a few days more, the same accident occurred in the same manner, and the pulsation never returned. On examining the artery at the seat of the accident, it was found that its inner coat had been lacerated and partially separated; and the canal of the artery at this spot was obliterated.

There appear to be on record but few cases of suppuration of the spleen; and therefore the following examples, from works which are not much read, may be worthy of insertion.

CASE XVII.—A young man, aged 18, several months after his recovery from a quartan intermittent, was affected with slight fever and violent colic pains. He had then loss of appetite and sleep, laborious breathing, œdema of the legs, scanty urine, and enlargement of the abdomen. After some time he was tapped, and purulent matter was drawn off to the amount of ℔iv., without any sensible diminution of the swelling. A second puncture was therefore made on the opposite side, and the same quantity of pus was discharged. He died next day. On *dissection*, the spleen was found so much enlarged as nearly to fill the abdomen; the other viscera lying behind it, very much compressed. It adhered extensively both to the parietes and to all the contained parts; and, being laid open, was found to form a sac, which still contained ℔vij. of purulent matter. It was about eighteen inches long and twelve inches in diameter; and the parietes of it varied considerably in thickness in different places; being in general about half an inch thick; in others so thin as to be nearly transparent.*

CASE XVIII.—A man (whose age is not mentioned) had been affected, for several years, with pain in the epigastric region, which was sometimes acute and sometimes obtuse, and a remarkable feeling of pulsation at the stomach. He had occasionally slight uneasiness in breathing, was easily fatigued by exercise, and a sense of suffocation was induced by any exertion. The pulsation at the stomach was increased by exercise, and by any excess in diet; and he had occasional vomiting. On examination, nothing could be discovered but a slight tension across the epigastrium. From the period when this account of his symptoms was taken, there was little change for ten or twelve months, except that there took place a slight yellow tinge of his skin. He was then seized with vomiting of blood, mixed with purulent matter; after which the pulsation at the stomach subsided greatly; food agreed better with him; and he felt easier than he had done for many years. But the vomiting returned in a fortnight, accompanied by great exhaustion; and he died in a third attack, after another week.—On *dissection*, the spleen was found adhering intimately to the stomach, and forming entirely a bag full of purulent matter and clots of blood. The parietes of the sac were in general about six lines in thickness; and it communicated with the cavity of the stomach, at the place of the adhesion, by an opening the size of a six Francs piece. It is not said that the spleen was enlarged in this remarkable case; and no disease is mentioned in any other organ. *

---

* M. L'Hermite, Hist. de l'Acad. de Sciences, 1753.

CASE XIX.—A woman aged 30, who had been long affected with a tumour in the left hypochondrium, was afterwards seized with fever, followed by pain in the abdomen; and after six weeks, a swelling appeared at the umbilicus, which was opened, and discharged several ounces of purulent matter. It continued to discharge for a month, when she died hectic. On *dissection*, the abdominal muscles were found livid and gangrenous, and a cavernous ulcer extended upwards betwixt the muscles and the peritoneum, so as to form a communication between the opening at the umbilicus and the spleen. In the substance of it, there was an abscess which had been the source of the purulent matter. † This singular case is described briefly; but it is obvious that the spleen must have adhered to the parietes of the abdomen on the anterior part, and that, at this adhesion, the abscess had communicated with the cavernous ulcer.

CASE XX.—A young man, aged 17, after several injuries from falls, had pain in various parts of his body, and general bad health. After several months, he had pain in the left hypochondrium, followed by palpitation of the heart, faintings, and great emaciation. The complaint was considered as a disease of the heart. He recovered a little after blistering, but soon relapsed, with severe pain, great palpitation, extreme debility, and evident swelling of the left side. After some time the pain ceased; he had then fetid and dark coloured stools, and soon after died in a state of extreme exhaustion about a year from the commencement of the disease. On *dissection*, the heart was found enlarged; and there was dilatation of the aorta. The spleen was as large as the head of a child of 18 months; it adhered to the colon, and contained in its substance an abscess which had burst into the colon at the place of the adhesion. The spleen, in other respects, was soft as if sphacelated.

A similar case is mentioned by Grotanelli; and another in which the abscess burst into the cavity of the abdomen. The patient, a girl of 12, felt immediate relief from severe pain which she had previously suffered, but died in three days. A man, mentioned by the same writer, was more fortunate. After various attacks of ague he had tumified spleen, which, after exposure to cold, became acutely painful, with nausea, vomiting, cough and fever. After some time the fever subsided; and after a month he was able to go about, but the swelling in the region of the spleen continued, and he had hectic paroxysms in the evening, and night sweats. In a quarrel, he now received a violent blow on the left side, after which the tumour of the spleen subsided, and he began to discharge thick and very fetid matter mixed with his urine. This continued about three weeks—he then recovered very good health, and had continued well for seven years, when the account was published.

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* M. Coze. Journ. de Med. Tom. LXXXII. p. 255.
† Heide, Centuria Observationum Med. Obs. XIII.
‡ M. Jacquinelle, Jour. de Med. tom, LXXXVIII. p. 360.

On reviewing the cases which have been mentioned, it is worthy of attention, that in those cases in which suppuration was found in the spleen, the disease had, in general, advanced in a slow and chronic form, while those in which the progress was more rapid, terminated by that peculiar black degeneration of the substance of the spleen which has been already referred to. I know not whether we are to suppose from this fact, that the spleen in its natural state is not liable to suppuration, and that the foundation for this termination is laid by tubercular disease, or by some change of structure which it undergoes by the previous induration and enlargement. The most common cause of induration and enlargement is intermittent fever. But it takes place slowly and insidiously without this cause, and the tubercular disease arises, as in other organs, without any obvious cause. A woman, mentioned by Grotanelli, had nausea, bad appetite, occasional vomiting, some cough, and pain in the left side;—she lost her colour, and the abdomen became tumid. The vomiting became more frequent, with quick pulse and anasarca, and she died in five months. On dissection, there was found considerable effusion in the abdomen. The spleen was enlarged, and contained twenty tubercles full of thick purulent matter.

When the inflammation is seated in the peritoneal covering of the spleen, the symptoms appear to assume more activity, and to vary considerably in their characters. In some cases, it assumes the appearance of pneumonia, or even carditis, but these varieties seem to depend in some measure upon the inflammation extending to the diaphragm. A man mentioned by Portal had dry cough, dyspnœa, pain of the left side, and most violent palpitation of the heart, with acute fever. He died after a short illness. The spleen was found extensively inflamed, and the inflammation had extended to the left side of the diaphragm. The lungs were sound.

There are other modifications of the symptoms, depending upon the particular organs which are affected either sympathetically or by the extension of the inflammation; such as urgent vomiting, colic pains or enteritic symptoms; but one of the most remarkable combinations is, when the disease extends to the kidney, and proves rapidly fatal, with the usual symptoms of *ischuria renalis.* I have seen one important case, which seemed to be of this nature, but I was not allowed to examine the body. There was first acute pain in the region of the spleen, with vomiting, the pain being increased by inspiration. At a very early period, the secretion of urine became very scanty, and after three or four days ceased entirely. In three days more the case was fatal, with coma, the pain in the region of the spleen having continued as long as the patient was sensible.

The spleen has been found lacerated by external violence, in some cases in which death happened rather unexpectedly. A young man mentioned by Lieutaud, died suddenly while boxing. Much extravasated blood was found in the cavity of the abdomen, and the spleen was lacerated on the side next the stomach. The same appearances were found in a boy mentioned by Tulpius, who received a blow on the left side while engaged in some youthful sports. He immediately complained of severe pain, followed by repeated faintings, and died next day. A man mentioned by Dr Chisholm, fell while carrying a burden, and struck his left side against a stone. He felt little uneasiness at the time, and next day was able for his work as a blacksmith. He was then seized with pain in the side, fever, delirium, and muscular spasms, and died on the 4th day from the injury. All the viscera were found in a healthy state, except the spleen, which was somewhat enlarged; and on the inferior surface of it, there was a laceration through its whole extent, and to the depth of two inches. The edges of the laceration were in some places florid, in others sphacelated.

Spontaneous rupture of the spleen occurred in a remarkable case by Fournier. A man had suffered from quartan ague for several months, but was considered as convalescent, when he died suddenly, after a hearty supper. The spleen was found enlarged and ruptured, and there was much coagulated blood in the cavity of the abdomen.

I have not entered on the subject of chronic enlargement of the spleen, having nothing new to offer in regard to it. It generally arises from intermittent fever, but may take place without that cause or any cause that can be traced. It varies in its structure, being in some cases a solid fleshy mass, in others studded with tubercles. In one case I found it consist entirely of an immense bag of hydatids, covered by the peritoneal coat of the spleen, the substance of the spleen being little altered from the natural appearance. All the forms of it are generally hopeless, but often continue long without much affecting the general health, and in a practical point of view, I believe the less they are meddled with, the better. Attempts to reduce them by mercury, are generally followed by the worst consequences. An enlargement of the spleen is said to take place in young women, in connexion with suppression of the menses, and to yield to a course of purgatives. I have not seen this affection, but I found it once enlarged and tender in a young man, and it yielded to the repeated application of leeches, with purgatives, rest, and cool regimen.

II.

Experiments on Variolous Inoculation, with Cases. Read before Dumfries Medical Society, 5th May, 1823. By WILLIAM MAXWELL, M. D., Dumfries.

NOVELTY, in all ages, has been, in its extended acceptation, the most powerful agent in the rational mind From the history of the apple to that of attraction, we have seen it the great excitement to mental exertions, the source of pleasure, and the introduction to the field of philosophy. It is the superlative of variety, the endless resource of the voluptuary, and the cheering stimulus of the philosopher. It is, however, much to be lamented, that it too frequently is imperfect in its results, and incorrect in its attributes. It has often so fascinating an influence on our perceptions, that the effulgence of the new light prevents us from distinguishing the imperfection of its combinations, seducing us by its brilliancy, from those less engaging courses that have been much better established by sober reasoning, and embodied by a more perfect concatenation of causes and effects. It is far from my intention to apply these observations, in their fullest extent, to vaccination; yet it must be allowed, that the world has been grievously disappointed, in the hope that this discovery would be a perfect security against variolous disease. This induces me, without making any observations on variola, varicella, or the new forms of eruptions, their combinations, modifications, varieties, or distinctions, to offer a relation of some experiments begun in 1801, and concluded in 1802, in hopes of giving a milder form to inoculated variola.

In 1801, the matter of small-pox was kept during a fortnight previous to its being inserted; no difference was observed in the time of progressive appearance of the eruption, from the usual habit of the disease. Matter taken from this subject was used at the end of a month, without any deviation from the usual symptoms. Inoculation was again repeated at the end of six weeks, as in the foregoing experiment, without any sensible alteration in the result. In experiment fourth, matter was taken from the last case and kept two months before insertion; when the patients, two in number, appeared to suffer less from fever; the pustules were earlier than usual; I thought they scabbed sooner, and had a narrower inflamed margin; in short, that the disease began to acquire a sensible degree of mildness. In experiment fifth, at the end of ten weeks, matter taken from one of the subjects in last experiment was used in several children; in some of them it failed to produce infection; where it succeeded, the disease appeared less active: the pustules began to scab on the sixth day. Experiment sixth was made with matter taken from one of the subjects of the preceding one; it was inserted at the end of three months; the eruption appeared on the fifth day with little or no fever; the scabs fell off sooner than in the last experiment, but in this there might have been some inaccuracy, as I had it only from report. Experiment seventh was made at the end of fourteen months; two subjects only out of nine who were inoculated, were infected; the eruption appearing on the end of the fourth or beginning of the fifth day, without fever or indisposition: the pustules were smaller, more prominent, like the small end of an egg, and almost entirely free from inflamed margin: the scab, of a foliated structure, fell off about the eighth day, without leaving any pittings. Four sets of children were now inoculated with this matter, applying it directly from the pustules: the disease in all respects resembled that in last experiment, the children playing about in a state of perfect health.

Although the periods of inoculation are said to have been at the ends of weeks or months, they were sometimes a few days sooner or later, but not varying more than four. This irregularity was occasioned by attention to my own convenience, as most of the experiments were done in or near the village of Newabbey, a distance of seven miles from Dumfries. I have not heard that any of the subjects of the above inoculation have been infected by either small-pox or chicken-pox, although they have both been in the village, and no means were taken to prevent communication with the infected. Here the author's investigation ceased, giving way to vaccination, which extended rapidly.

At the time these experiments were made, we were not generally acquainted with that nice discrimination of eruptions with which modern writers have furnished us, and I have been obliged to confine myself to a very imperfect description of symptoms, drawn from such notes as my rapid visits allowed me to take.

Dumfries, 5th May, 1823.

III.

Observations on Hydrocephalus Internus, with Cases. Read before Dumfries Medical Society, 4th November, 1822. By William Maxwell, M. D.

The following communication consists of observations on the treatment of that state of the brain expressed by a coincidence of symptoms which are generally found after death to have accompanied a deposition of water in the brain or cerebellum, but which occasionally prevail without effusion of fluid; death appearing in those cases to have been occasioned by inflammation of the substance of this organ, or of its membrances, or by abscess, hydatids, encysted tumours, &c. I am, from this circumstance, disposed to use the term Hydrocephalus Internus, more in an abstract sense than as an expression strictly derivative.

In the year 1795-6, previous to beginning regular practice, I saw about twenty-five cases of hydrocephalus internus, all of which terminated fatally. From this it was evident that the modes of treating this affection were inefficient, and that hopes of removing the disease must be founded on some new or more active treatment; as in many of the above cases the patients had in an early stage been very freely purged, bled with leeches, and in the arm; and in others a diarrhœa had prevailed from the first attack, and continued to an advanced period of the disease. It was thought proper to employ very copious blood-letting in the way by which the largest quantity of blood could be rapidly abstracted from the head: this mode of treatment exceeded expectation, and was done in the following manner: And, as there was considerable danger of the patient sinking from inanition, it was thought a necessary preliminary to obtain the full consent of the parents to a measure attended with much hazard, and extremely painful to the feelings of the practitioner, by informing them, that after the usual mode of purging, moderate bleeding, &c. this distressing measure was the only mean by which the life of the patient could be preserved. I shall, out of nearly ninety cases, about sixty of which recovered, select two wherein the usual characteristic symptoms of the disease were distinctly marked.

Master J., æt. 7, a healthy boy, became dull and indisposed to his usual exercise, complaining of headach, which, with much langour, increased during six or eight days. The bowels were frequently moved by purgatives. At this period pain in the head became more distressing, and the bowels were difficultly moved. The boy showed no inclination to leave the house;—he rested frequently his head in his hands upon a table, or in a kneeling posture on a chair; the muscular power of his limbs began to fail; the pulse became rapid; pain of the head excessive, with occasional remissions, and during these, constant drowsiness came on. Still, however, there was a considerable disposition to take food, although it was passed half digested. The mind now became indistinct, with inability to articulate; vision was imperfect; the evacuations took place without his attention; squinting next appeared; pulse 160—frequent expression of pain in the head; muscles in the neck became supple, the head rolling upon the breast and shoulders, with immobility of the pupils and total want of expression in the eyes; the face pale and inanimate. Mr J., his father, a medical gentleman, requested that I would take the sole direction of the case, observing, that he was well aware that extreme bleeding was the only means by which the child could be recovered; desiring that it might be carried to whatever extent it might be thought necessary, and that, if the child should sink under such active treatment, he would still retain a warm sense of gratitude for the painful task which he had imposed on me. The time was fixed for the operation, and, with the father's consent, I invited my medical friends Dr G., Mr S., Mr M. and Mr St., to be present. The father, after providing every thing necessary, withdrew to a remote part of the house, waiting, with the anxiety of an affectionate parent, the fate of an only son.

The boy was laid on a mattress, his head somewhat lower than the rest of the person, medical gentlemen holding each wrist; the jugular vein on the right side was opened; it bled rapidly—the stream was frequently interrupted to prevent fainting. The bleeding was continued till syncope began to take place: a little negus was then given; when the pulse revived the finger was removed from the orifice and the blood allowed to flow till the gentlemen agreed that the pulse could no longer be felt. The patient at this time had no appearance of life, and continued without the least symptom of animation for ten minutes, when he began to revive gradually: and in the evening more favourable symptoms appeared. His mind was remarkably improved, as well as his physical powers, being now able to articulate, although indistinctly, and to tell the hour on a watch. He had a tolerable night's rest, having taken frequently a little water-gruel and beef-tea. During the following day there was but little improvement. On the third day the bleeding was repeated in the left jugular vein, and a complete recovery followed.

CASE II.—In April 1816, I was called to see a child of Mr B. ætat. 1¼. He had been affected with symptoms of hydrocephalus about seven days, had been repeatedly purged, and bled profusely on the head by leeches. I found the child very languid, insensible to moderate pressure or pinching; vision appeared to be entirely gone, eyes death-like without any expression; squinting with both eyes had continued during three days; uttering frequently shrill screams; the head rolled upon the shoulders as if it had been attached to the trunk by muscle alone. He was bled in the neck, as in the last case, till the pulse could no longer be felt, and remained in this state during some hours before he began to revive. I left him at 11 *p. m.*, and was so apprehensive that bleeding had been carried too far in this case, that I sent my servant next morning to enquire what had happened during the night, and was surprised by learning through him, that the child was so much better, that he saw him at the window in the nurse's arms, looking at two young cocks fighting in the yard. He recovered daily, and is now a fine healthy boy.

In upwards of ninety cases that have been treated in this way, above sixty have recovered. In most of these I have had the assistance of one or more medical gentlemen of this place. Bleeding was generally repeated, as the constitution could seldom bear at one operation a subtraction of blood sufficient to remove the malady.

It may fairly be objected to the success of this treatment, that hydrocephalus internus did not exist in the above sixty cases, but that other causes might have produced delusive symptoms. I confess that I cannot give an answer sufficiently satisfactory, and that I cannot meet the objection by any better proof than by analogy, for, owing to the aversion that prevails in this part of the country to the examination of the dead, it was with much difficulty that I was allowed to inspect seventeen or eighteen heads out of the twenty-seven. In all of these, with the exception of eight, from one to four or six ounces of fluid were found in the ventricles or within the membranes. As the cases of recovery did in almost every symptom resemble those that terminated fatally, where the heads were allowed to be opened, we may analogically be allowed to infer that the disease in both was the same; and, without better proof, to consider such close analogy a legitimate conclusion. In the other eight cases, above alluded to, the quantity of water did not exceed half an ounce. In the first of these, a cyst, containing about four ounces of fluid, communicated with the right lateral ventricle: death took place at the end of seven months. In the second, there was suppuration in the middle of the right lobe. In the third, three round tumours of indurated cerebrum were found in the cerebellum. In cases fourth, fifth and sixth, much inflammation of the membranes and substance of the brain had existed. In the seventh, suppuration in the *thalami nervorum opticorum*. And in the last, excessive inflammation of the membranes, with some pus, was perceived near the *foramen magnum*.

Dumfries, 4th Nov. 1822.

IV.

Case of Rupture of the Uterus Terminating Fatally. By P. Mudie, M. D., St Andrews.

Mrs Eliz. Nickoll, ætatis 32, was taken in labour early in the month of May 1819, and after 48 hours was delivered by me, by the forceps, of a still-born child. She was a short thick made woman, with a very narrow pelvis; as the distance from the sacrum to the pubes did not appear to be above three inches. She made a good recovery, and was employed as a nurse. She again fell pregnant, and I was again under the necessity of employing the forceps, on the 24th March 1821. The child breathed, but soon after expired. In her next pregnancy, after a severe labour, a small living child was born by the natural efforts on the 21st of April 1822. On Friday 20th March last, at 9 o'clock in the morning, I was called to the same patient, and was informed by the midwife that she had been with her since 6 in the morning, that the *os uteri* had dilated regularly, and that the membranes had given way about half an hour ago; when, with the waters, about 12 inches of the cord had been protruded. Upon examination, I found this to be the case, but the pains were so severe, and the head so firmly pressed against the brim of the pelvis, that I could not return the cord. The head was presenting naturally, and I found it gradually entering the pelvis. As it was impossible to return the cord, and as I knew from former experience, that I could not hope to save the child by turning, I placed the funis as nearly as possible in the cavities at each side of the sacrum, in hopes that it might escape that pressure which would destroy the child. But I soon found that hope to be unavailing, for the pulsation of the cord began to be very feeble, and before twelve o'clock it had entirely ceased. The pains were very severe; and as the patient complained of excessive pain in the left side of the abdomen, and her pulse was full, I took away about twelve or fourteen ounces of blood. As there was now no doubt of the death of the child, I had determined to open the head; but knowing the difficulty that would attend the delivery, even in this case, I resolved to wait a short time longer, that the dilatation of the parts might be advanced as far as possible by the natural pains. I directed the midwife to give her an emollient injection, and left her about one o'clock. About an hour after I was sent for, and found the circumstances very much changed. The midwife informed me that the injection had operated, and that she had had several severe pains, which had made the head advance considerably, when suddenly, during a pain, she cried out that something had given way, and that the pain in the side had become intolerable. The midwife examined, and found the head completely receded, and that there was a considerable discharge of blood. Upon examination, I found the statement of the midwife correct, and upon passing my hand over the abdomen, I could not perceive the uterine tumour, but a confused pulpy swelling. The labour pains had entirely ceased, but the pain of the abdomen was so severe, that she could not allow it to be touched. She was gastly and sick, with almost incessant vomiting. Suspecting that a laceration of the uterus had taken place, I immediately requested the attendance of my friends, Drs Briggs and Luscombe. The former arrived in a few minutes, and we immediately resolved to deliver by the feet. I had no difficulty in doing so, for there was not the slightest resistance to the introduction of the hand. The child was of large size, and I had great difficulty in bringing down the body. After the arms were brought down, I introduced the perforator at the lambdoidal suture, and evacuated the contents of the cranium; I then introduced the crotchet, and fixing the hook at the *foramen magnum*, was able, by the assistance of Dr Briggs, to bring the head away. I introduced my hand into the vagina, and finding the placenta lying loose in the cavity of the pelvis, I brought it easily away. I again introduced my hand, and felt that it proceeded into the cavity of the abdomen by a rent in the anterior part of the *cervix uteri.* I felt the uterus contracted, and lying towards the spine, resembling in some measure another placenta, and the cavity of the abdomen anterior to it seemed to contain nothing but clots of blood. There was very little flooding, but the patient seemed sinking, and she complained much of the pain of the abdomen. We endeavoured to revive her by cordials, and after applying a bandage firmly to the belly, we gave her an opiate containing forty drops of laudanum. The delivery was completed about half past three o'clock.

Half-past 4 o'clock. She has been very uneasy, and has vomited several times. Pulse very feeble; there has been a considerable discharge from the vagina, but it is more watery than usual.

Half-past 6.—Pain rather abated, as has the discharge; no vomiting; pulse rather firmer; the body is bedewed with perspiration.

10 *p. m.*—Feels easier; little or no discharge. She has been able to be shifted, and has had her abdomen fomented with hot vinegar and water; very little discharge; she has passed a little urine. *Repet. haust. anodyn. cum tinct. opii gt. xxv.*

21st.—8. *a. m.*—Has passed a tolerable night, and as she experienced considerable relief from the fomentation, it was repeated during the night. She is now perspiring freely; pulse feeble and excessively quick; thirst very urgent. *Injiciatur q. p. enema domestic.*

Let her have freely of acidulated gruel, or weak tea for her common drink.

1. *p. m.*—Continues tolerably easy; pulse 150, and very feeble; thirst very urgent; perspiration profuse; little or no discharge. Enema has not operated, but she has again passed urine.

6. *p. m.*—Since the last visit, there has been a considerable increase of pain and tension of the abdomen. She has vomited frequently; complains much of thirst; tongue white; pulse 140 and full. She was immediately bled to 19 or 20 ounces, when she became faint.

> *Repetat. fotus abdomini. Sumat. infus. sennæ ℥i. cum sulph. magnes. ʒi. omni hora.*

10. *p. m.*—Has vomited the infusion, and indeed nothing has remained upon her stomach; what she brought up is extremely acid.

> *Sumat. magnes. (calc.) ʒi. statim, et repetatur post horas tres.*

22*d.*—6 *a. m.*—Passed a very restless night; pain of abdomen much increased; pulse 140, and full. She has retained the magnesia, but there has been no effect on her bowels. She was immediately bled to the extent of 24 ounces, when she became faint.

Repet. magnes. ʒi. et repetatur post horam.

Let her have whey to drink, and oranges.

Injiciatur statim enema domestic.

10 *a. m.*—Has been rather easier. She has retained the magnesia; no stool.

℞ *Submur. hydrarg. aloës a. a. gr. x. Conserv. ros. q. s. ut ft. massa in pil. No. iv. dividenda. Sumat unam omni hora.*

3 *p. m.*—Has retained three of the pills, but without effect. Pulse 140; considerable pain in the abdomen; complains of cramps in the thighs.

Repet. fotus abdomini. Repet. pil. ut antea. Fricentur femora et crura tinct. saponis et opii.

6 *p. m.*—Has retained two more of the pills; pain of abdomen increased: pulse full.

V. S. ad ℥xviij.

10 *p. m.*—Pain of abdomen very severe; no vomiting; pulse still full.

Repet. v. s. ad ℥x. Injiciatur enema domestic. Sumat. liquor. opii sedativ. (Battley's) gtt. xx.

23*d.*—6 *a. m.*—The injection came away, but without any mixture of feculent matter. Pulse very frequent, but rather softer; great tension of the belly.

Admoveantur abdomini statim hirudines octodecim, et postea fotus.

11 *a. m.*—Has passed a considerable quantity of clay coloured feces; feels rather easier, and the tension of the abdomen is rather diminished; there has also taken place a considerable watery discharge from the vagina, which is scarcely tinged with blood; pulse very frequent, but softer; vomiting abated.

Repet. fotus abdomini; et si dolor abdominis maxime urget, admoveantur iterum hirudines viginti.

5 *p. m.*— Pulse softer; pain of abdomen rather abated.

Repet. liquor opii sedativ. ut ante.

10 *p. m.*—As the pain of abdomen had rather increased, fourteen leeches were applied; she has had several fetid stools; frequent vomiting; pulse very feeble.

Repet. haust. anodyn.

24th.—3 *a. m.*—Vomiting continues; pulse scarcely to be felt; extremities cold.

Sum. statim. opii gr. iss.

Vomiting of black matter continued, with little intermission, till she expired at 6 *a. m.*

Dissection.

25th.—*Noon.*—Upon laying open the abdomen, the intestines were observed covered with blood and coagulable lymph, and bore marks of severe inflammation, being adherent to each other throughout the whole extent. The uterus was found nearly of the usual size at such a period from delivery, of a blackish hue, and a rent was found in the anterior part of the cervix, extending transversely nearly one half of its extent. The abdomen contained a great quantity of bloody serum. The pelvis was of small size, but not deformed.

St Andrew's, 6th April, 1824.

V.

Notices of certain Accidents and Diseased Structure of the Caput Cæcum Cœli and its Appendage. By H. HOME BLACKADDER, Surgeon, Edinburgh.

A STOUT healthy looking man, about forty years of age, suddenly fell to the ground while standing in the erect posture, and engaged in conversation. There was no contortion of the features, nor convulsive motion of the limbs. The eyes were closed—the face rather pale—breathing short and hurried—the pulse a mere flutter, scarcely to be felt. Several attempts at general blood-letting were made almost immediately, but with very little success. Within an hour after his being thus seized, I saw him for the first time. His eyes were still shut, and he paid no attention to questions that were addressed to him; but now and then he moved different parts of his body as if from pain, and his countenance was expressive of severe oppressive suffering. The respiration and pulse as before. On closer examination, it was found that the abdomen was greatly and permanently retracted, the respiration being performed solely by the thorax. On touching the abdomen with the hand, he instantly gave signs of severe suffering by writhing his body, moving his limbs, or rolling his head from side to side on his pillow. Still his eyes remained shut, and he did not attempt to speak. It was now remarked, that every other while he had a slight shivering fit, as from a sensation of cold; and it was very soon evident that this symptom was on the increase. Fomentations were applied to the abdomen and inferior extremities. A warm bath was ordered to be got ready, and an ethereal draught was with difficulty administered, but which fulfilled the purpose for which it was chiefly intended. He now became more restless. His eyes occasionally opened, and he attempted to speak, but for some time could not articulate. At length, and with much apparent exertion, he could answer such questions as were put to him, but seemed so overcome with oppressive suffering, that he never attempted to speak unless when urged to do so. I learned from him that he experienced a most excruciating pain of the abdomen, which was greatly aggravated by even slight pressure; and he earnestly begged that it might not be touched. The fits of shivering became more severe and frequent, so as to be almost continual; the pulse still small and fluttering. Castor oil and injections were administered, and the contents of the bowels were found to be perfectly natural. He was now placed in a warm bath, which at first seemed to give relief. The cold sensation and shivering were removed; and the pain of the abdomen was, he said, mitigated. It was evident, however, from the expression of his countenance, and the state of his pulse and respiration, that no great alteration had been produced. After remaining in the slipper bath about ten minutes, he suddenly requested to be removed to his bed; and this was hardly effected when he expired.

I have no hesitation in confessing, that I was much embarrassed by this case. The whole period of his illness did not much exceed three hours and a half, and I could not, at the time, form any satisfactory opinion in regard to the *ratio symptomatum.*

I examined the body twenty-four hours after death, and the following morbid appearances were detected.

The heart was rather large, its parietes thick and firm, its cavities on the right side were gorged with blood, and in the left ventricle there was a reddish amber-coloured coagulum, from which a small branch extended nearly to the arch of the aorta, and terminated at the commencement of a shallow scaphoid dilatation of that artery, at the upper and anterior part of its arch. This dilated portion of the aorta did not exhibit any ossification, rupture or separation of its fibres, nor had the surrounding parts suffered any apparent change of structure. The valves of the aorta were somewhat thicker and firmer than natural.

The abdominal viscera had an unusually healthy appearance, except the *appendicula vermiformis,* which was remarkably increased in length and thickness, and felt like a firm chord between the fingers. On making an incision through its coats, this elongation and distention were found to be produced by a very large lumbricus, which had forced its way into the cavity of the *appendicula*, a portion of its tail, of about an inch in length, still projecting into the *caput cæcum*. There were no more lumbrici contained in any part of the alimentary canal, neither did its serous or mucous membranes exhibit the slightest marks of their having suffered from either former or recent irritation.

Was the presence of the lumbricus, and its peculiar situation, merely accidental, and in no way connected with the symptoms? or was it the direct and primary cause of them? For my own part, I have little doubt that the *appendicula* was the original focus of irritation—that the irritation there excited by the lumbricus was productive, either directly through the connexions of nerves, or indirectly through the spinal chord, of the spasmodic action of the abdominal muscles, &c.; and, that the permanent contraction of these muscles, by impeding the motions of the heart and lungs, and by producing an overflow of blood to the thoracic and cranial cavities (the heart and aorta being previously, and more or less diseased), was the cause of the circulation becoming interrupted, and consequently of the death of the patient. Several, and, as they appear to me, conclusive reasons, might be adduced in support of this view of the question; but you will probably be disposed to think that the subject has had its full share of your attention. I shall, however, seize this opportunity of remarking, that diseases of the heart are certainly far more frequent than seems to be commonly understood; the symptoms which accompany them being but too commonly referred to the comprehensive *Dyspepsia*, or to the (if possible) still more indefinite "*Nervous weakness;*" and only, if at all, recognised when the unfortunate patient can no longer rationally count on a momentary prolongation of his life. The frequent consequences of this misconception, and consequent virtual neglect of the early symptoms, are, that the often highly susceptible feelings of the patient are cruelly lacerated, his real sufferings increased, and the progress of his disease accelerated, at the very time when active medical treatment is of the first importance, and when alone it can prove of permanent advantage. In support of what I have now advanced, I might adduce instances (more than one or two) that have come under my own observation, in which eminent physicians, and some of the first eminence, were very decided in their opinions—very completely mistaken—and very speedily had, or might have had, a melancholy opportunity of correcting their erroneous decisions. If, then, such things happen with the roots and trunks, what is to be expected from the innumerable branches?

In returning from this long and unpremeditated digression, I shall curtail, as much as possible, the following notices.

In one instance, I met with a calculous concretion in the *appendicula vermiformis*, about half an inch from its orifice. In size and shape it resembled a thrush's egg. Externally it had a thin coat or crust, of a greyish coloured earthy looking matter, and internally its structure resembled a ball, formed of minute ligneous fibres. This patient died of ulcerated and tuberculated lungs, in connexion with tuberculated induration of the liver; and was never heard to complain of any uneasy sensation at the head of the colon, or in its vicinity. *

In another case, in addition to various other chronic ailments, in which the lungs and liver were more especially implicated, the patient complained of a constant gnawing soreness about the head of the colon. On examining his body after death, I found the inner surface of the *caput cæcum* covered with ulcers, having ragged and irregular edges, and which might have been aptly described by the term "worm-eaten." On the surface, or under the edges of each of these ulcers, were found several large ascarides; but neither any of these animals, nor any morbid appearance, could be detected elsewhere, in the whole course of the alimentary canal. It may be a question, whether the ascarides were the original cause, or only the accidental promoters of the ulceration; but there can be no doubt that such ulcers could not acquire a healthy action, as long as their surfaces were irritated by such a cause.

Another patient had long been affected with a disordered state of the stomach and bowels, and complained of a pretty constant pain, soreness or uneasiness about the head of the colon, and which was not particularly increased by pressure. Various modes of treatment had been resorted to without very obvious or lasting benefit. At length an attack of jaundice supervened, and which continued some length of time, without any very marked alleviation being produced by the different means that were employed. One day, all at once, and very unexpectedly, this patient passed by stool an enormous quantity of *ascarides*, mixed with a glairy mucus, in which they seemed to have been imbedded. It did not appear evident that this dislodgement was the effect of any particular medicine, but from that day the patient recovered rapidly; the pain of the side was gone, and the yellowness of the skin very quickly disappeared.

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* Concretions are sometimes met with in the blind head of the colon, and even so large as to weigh 4 lib. Sometimes two are present at the same time, and have been detected during the life of the patient, by the noise produced when they were made to strike against each other. They have been found to occasion much pain, and ultimately to be the cause of death. Dr Monro *secundus* used to demonstrate the possibility of making an incision into the *intestinum cæcum*, without perforating the peritoneum; and for the purpose of extracting these concretions, which, he stated, usually proved fatal when they attained the size of a hen's egg. If ever this operation is to be performed, the attempt is most likely to be made at the present day; and it may therefore be of some importance to know, before resolving on such a step, that sometimes the concretion is lodged in the *appendicula*.

Independently of other strong objections to the use of calomel, more especially in such subjects, as intestinal worms are most commonly to be met with, I have never found it so certain in its effects as a suitable dose of aloes, along with an alkaline solution; administering also a large injection of tepid lime water, before the operation of the purgative has extended to the understood site of the ascarides. By a little address the lime water injection may be made to reach the head of the colon; and, so far as I have learned, these worms have never been met with above the valve of the ileum. This mode of treatment, I have learned, has been put in practice by others, and with success. I was first led to it many years ago, as the result of a very hypothetical speculation. What are termed vinegar eels and the ascarides, seemed to bear a resemblance to each other, both in their external appearance, and in the circumstances of their production. The former make their appearance only at a time when the vinegar has begun to undergo a decomposition of some kind or other, one of the effects of which are the formation of a thick slimy scum or sediment. When these animalcules were large and vigorous, they were not, I found, deprived of life by ardent spirits or even turpentine; but were instantaneously killed by a weak alkaline solution, such as lime-water. The ascarides are almost never met with in the human body, except in connexion with a very obviously acid and slimy state of the contents of the intestines, and only in that part of the alimentary canal where the contained matters, though still more or less acid, have in part undergone the putrefactive fermentation. It would be a rash inference to draw from these perhaps imaginary coincidences, that vinegar eels and ascarides have any thing common in their origin. Experiment proved, however, that while immersion in spirits, &c., is immediately fatal to neither, weak alkaline solutions are equally and instantaneously destructive to both.

The foregoing notices of cases, &c., are not brought forward from an idea that they contain any thing very remarkable or singular. Unique or extraordinary occurrences excite curiosity, and may be worth knowing; but the object I had in view was to draw the attention to some of the occasional causes of those obscure symptoms which but too frequently present themselves, and of which it is often so difficult to determine, previously to the death of the patient, the true nature or origin, and consequently the proper treatment.

Edinburgh, 31*st March* 1824.

VI.

Observations on the Anatomy of the Lacteal System in the Seal and Cetacea. By Dr KNOX, F.R.S.E. &c. Communicated to Dr DUNCAN, Junior.

MY DEAR SIR,

AGREEABLY to your wishes, I herewith send you a brief account of my dissection of the lacteal system in the seal, and have prefixed to it a short history of the circumstances which led to the inquiry. Many, perhaps, may be inclined to think, that the error which those great anatomists, Tiedemann and Fohmann, have fallen into, relative to the anatomical distribution of the lacteal vessels, and consequently the formation of the thoracic duct, might have been foreseen *a priori*; but the authority of these anatomists, and more particularly of Tiedemann, is such, that many naturalists and physiologists would have demanded a direct demonstration, and the question might thus have remained for some time problematical. The preparation I had the honour to show you lately, seems to me to decide the question for ever, and to leave no doubt whatever as to the incorrectness of the opinion of Tiedemann.

As I received from you, who are so well acquainted with German literature, the first hint on the subject, and indeed, as the inquiry was suggested by yourself, I beg of you to accept of the following observations,—and to believe me always,

My dear Sir, your's very truly, R. KNOX.

To Dr Duncan, Jun. &c. &c. &c.

1.

PREVIOUS to certain dissections by Professor Tiedemann and Dr Fohmann, no one had doubted that the absorbent vessels conveying the chyle from the small intestines, and generally called *lacteals*, proceeded, in the seal as in other animals, to the mesenteric glands, into which they penetrated, and in which they terminated; from which glands certain other vessels, in structure quite analagous to those just described, arose, which vessels terminated in one or other of the branches of the thoracic duct, contributing very materially towards the formation of the duct, and of the *receptaculum*, or *cisterna chyli*, placed usually at the commencement of the duct, and just where it passes through the diaphragm. The first set of vessels, those conveying the chylous fluid from the intestines to the mesenteric glands, anatomists have agreed to call *vasa afferentia;* whilst the second series, or those carrying the chylous fluid (whether the same or altered) from the glands into the thoracic duct, have generally been called *vasa efferentia.* The existence of these latter vessels in the seal has been denied by Tiedemann, who states, that the chyle, poured into the glands by the *vasa afferentia*, is absorbed by veins only, arising from these glands, and thus conveyed directly into the blood, without passing along the thoracic duct. It would follow, were this account of the anatomy of these vessels correct, that the thoracic duct in these animals was intended for carrying the lymphatic fluid only, and that the chyle passed into the general circulation by quite another route;—but nothing can be imagined more incorrect than such a statement. In the specimen of the common seal (Phoca Vitul. Linn., Seehund of the Germans), which is now lying before me, and from which this decription is taken, I find the lacteal vessels proceeding from the small intestines to be very numerous, distinct, and injected with chyle;* the greater number of them proceed towards a large mesenteric gland, about four inches in length, placed close to the spine, immediately behind the turn at the duodenum, and not far from the pancreas; the remainder of the lacteals pass into two smaller mesenteric glands, situated close to the former, but somewhat lower down. Into these glands the lacteals penetrate, dividing very generally into several branches before dipping into the gland. Near to the lower or smaller end of the largest mesenteric gland, are seen several branches of lacteal vessels (in the present specimen also loaded with chyle), arising from the gland, and speedily uniting into a larger trunk. This, in its course upwards towards the larger or diaphragmatic end of the gland, is speedily joined by many others from every portion of the gland; these united, form a large trunk, lodged in a fissure which divides the gland as it were into two, longitudinally. By the time the duct has reached the larger extremity of the gland, it is of very considerable magnitude, exceeding considerably a crow's quill, and possessing a valvular structure. After quitting the gland, it crosses over towards the right side, and mainly contributes to form the *receptaculum chyli*, constituting indeed one of its largest branches.†

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* The animal was killed only two days ago.

† On laying open this large and important *vas afferens*, which was done throughout a considerable extent, it was observed, that the vessels joining it from the mesenteric glands had their orifices uniformly leading in one direction, *i. e.* in the

The *receptaculum chyli* in the seal is very large; it is situated close to the aortic passage of the diaphragm, and may be said to be formed chiefly by two large branches. One of these, the lacteal branch, (the real *vas efferens* of the mesenteric glands), has been already described; the other is situated more towards the right side, proceeding from the loins and lower extremities. I traced it readily to the pelvis. Several other branches, but somewhat less in calibre, join the *receptaculum* from the side of the stomach and chest. The thoracic duct is large and regular; it joins the general venous circulation as usual, by entering the veins near the junction of the jugular and subclavian of the left side. The fluid contained in the thoracic duct was much thinner and less opaque than the chyle, which filled as well the *vasa afferentia* as the *vasa efferentia.*

The dissection was one of the easiest I have ever attempted; nor can I account for the extraordinary oversight of Professor Tiedemann in any other way than by supposing his specimens to have been greatly mutilated, and totally unfit for dissection. The parts have been prepared for demonstration, though, from the great facility with which the true anatomy was made out, I scarcely deem them of that importance which otherwise would have belonged to them, had they, instead of merely correcting an error, established any new fact in anatomical and physiological science.

19th March, 1824.

2.

In a letter which I had the honour to submit to you on the anatomy of the chyliferous system of vessels in the seal, (*Phoca Vitulina*, *Linn.*), I endeavoured to show, that whatever support the modern doctrine of "absorption by means of veins" might have derived from the supposed discoveries of Professor Tiedemann and others relative to the distribution of the lacteal system in the seal, such support must, for the future, be denied these doctrines, for the reason, that a dissection, performed as you have seen, before numerous competent judges of such matters, and under the most favourable circumstances, entirely disproved the discoveries of the distinguished German anatomist, and set the question, as I think was likewise your opinion, at rest for ever.

Permit me briefly to mention to you certain details which

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course of the chyle, and towards the *receptaculum chyli*, in which direction only the fluid could pass, by reason of the numerous valvules of the *vas efferens* and thoracic duct.

ought perhaps to have been introduced into my former letter, but which the comparative facility of the dissection, with such as, in the course of my comparative anatomy studies, I have been accustomed to perform, induced me to withhold.

The seal I examined had been very recently taken in a net by the fishermen in the immediate neighbourhood, so that it came into my hands in the freshest condition. The lacteal vessels of the animal, as far as the mesenteric glands, that is, the vessels usually called *vasa afferentia*, were filled with the chyle, the fluid naturally belonging to them. The great *vas efferens*, into which poured numerous smaller ones, congregating as it were from every portion of the gland, was also filled with chyle; it entered, by a very large trunk, the *receptaculum chyli*, which in the specimen I examined was comparatively large; nor had the chyle in the least altered its sensible qualities till after it became mixed with the thinner lymph, brought into the *receptaculum*, by several other great branches of lymphatic vessels which entered it, coming from the posterior extremities, loins, back, &c., and which have been already described.

Hitherto it might be said, that the knife had scarcely been used, and in this perfect and natural state, the dissection was shown to you. Previous to putting up the parts in spirits as an anatomical specimen, I filled the lacteals proceeding from the intestine (which, for brevity's sake, I shall hereafter call the *vasa afferentia*) with quicksilver; the metal penetrated the largest of the mesenteric glands, and seemed to reappear in the *vas efferens* in its way to join the thoracic duct, but this I afterwards found not to be the case. I beg of you to remark here, that there were filled with quicksilver in the seal about twenty lacteals, *and lacteals only*, for I injected those vessels alone which contained chyle. I recommend this caution strongly to the attention of those engaged in such inquiries. It has appeared to me, that after a time the lacteals empty themselves of their natural fluids, and in this state the minute branches of the mesenteric veins bear a considerable resemblance to them. It was not thought prudent to push these injections too far, lest the vessels should rupture, and the preparation (which is to be placed in the Anatomical Museum of the Royal College of Surgeons) be destroyed. Finally, previous to placing the preparation in spirits, the *vas efferens* was opened, and, on gently blowing air into it, the thoracic duct was immediately filled as high as its passage into the veins.

I conclude then, that, if ever quicksilver found its way into the mesenteric veins of the seal, on being simply injected into the lacteals proceeding from the intestine to the mesenteric glands, that such an appearance by no means marks the usual route of the chyle into the general circulation; for we have shown, 1st, that there exists at least one large *vas efferens*, of a calibre amply sufficient to convey the whole of the chyle into the thoracic duct; 2d, that this vessel, into which are poured innumerable smaller branches from every part of the mesenteric glands, is, in the healthy and fresh specimen, filled with a fluid resembling chyle in all respects; 3d, that although quicksilver did not, at least satisfactorily, pass into this vessel by being injected into the *vasa afferentia*, we have no reason to think that it may not do so in the very next specimen examined. Moreover, the *vas efferens* was filled with its own fluid the chyle, which I imagine to be much more satisfactory than if filled with quicksilver; and finally, as we shall see in the next letter, how trifling a pressure will stop the progress of quicksilver through any vessel, no important inference can be drawn from its appearance or non-appearance in the thoracic duct, unless supported by more direct testimony, drawn from the true anatomy of the part. I might add, that I have great doubts whether the *vasa efferentia* in man can be injected by pouring quicksilver into the *vasa afferentia*. Who would venture to call their existence into question? Yet we have no other proofs of their presence than what I have offered in the seal, viz. their demonstration by dissection; their direct continuity with the thoracic duct; the origin of their roots in the mesenteric glands; lastly, *their injection with chyle.* *

But there is a class of animals in which the phenomena attending the passage of the chyle into the mass of blood, differ in several respects from what occurs in man and in other mammiferous animals; and it has been suggested to me, that perhaps Professor Tiedemann's views might have been directed chiefly to the class of animals I now speak of, viz. the cetaceous mammalia.

As early as 1795, my much respected preceptor, Mr John Abernethy, demonstrated, that in the whale (the specimen was a male of the *genus* balæna) there are two ways by which the chyle can pass from the intestines into the thoracic duct; "one of these is through those lacteals which pour the absorbent chyle into bags (mesenteric glands), in which it receives an addition of animal fluids. The other passage for the chyle is through those lacteals which form a plexus on the inside of the bags; through these vessels it passes with some difficulty, on account of their communications with each other; and it is conveyed by them to the thoracic duct, in the same state that it was when first imbibed from the intestines." *

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* I do not expect that physiologists shall set aside the usual arguments in favour of venous absorption; but the presence of a *vas efferens* in the seal, and its great size, and, moreover, the facility with which it may be seen, render the opinion extremely improbable, not to use a stronger expression, that the chyle enters the mass of blood by any other route than that of the thoracic duct.

The peculiarities in the anatomy of the lactiferous vessels of the whale, as discovered and first described by Mr Abernethy, are important. There are *vasa afferentia* which are continuous with the *vasa efferentia*, and with the thoracic duct. † But as the lacteals open into the interior of the mesenteric glands (which are in this animal hollow internally) by sufficiently wide orifices, and pour the chyle into a central cavity, into which also enter numerous veins and arteries, also by large and distinct orifices; it must be evident, that when quicksilver is poured in sufficient quantity into these cavities through the *vasa afferentia*, it will more readily find its way into the general venous system than into the thoracic duct, by reason of the smaller size of the respective vessels. But though this happens to quicksilver in the dead body, every physiologist knows, that a precisely inverse law may take place in the living; and it is sufficiently remarkable, that in the Memoir of Mr Abernethy on the Anatomy of the Whale, that eminent physiologist, though he found numerous orifices of veins entering into the central cavities of the mesenteric glands in that class of animals, does not therefore infer that the chyle passes into these veins; on the contrary, he infers that there are *vasa efferentia* which absorb the chyle so poured out; but it is matter of regret, that the existence of such vessels was not proved by him.

I became very anxious to ascertain if such an anatomy of the mesenteric glands and lacteals as had been discovered by Mr Abernethy, existed also in the common dolphin or porpuss; for I had been assured by a friend, that he had seen a specimen in which the quicksilver, injected into the *vasa inferentia*, passed readily into the mesenteric veins, and so into the general circulation. In the first specimen I examined, I observed that quicksilver did pass from the mesenteric glands into the *vena portæ*, by means of a small vein, though not in any great quantity. But this did not happen until the animal had lain nearly three days on the dissecting table, in very sultry weather, and until I had injected rather more than sixty lacteals.

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* Phil. Trans.

† I suppose them continuous with the thoracic duct; but it is to be remarked that they were not traced to that vessel by Mr Abernethy. Neither is it stated, in the Memoir alluded to, whether or not any lacteals were observed to arise from the cavities contained in the mesenteric glands, and proceed to join the thoracic duct; these are important points in the discussion, and merit a farther minute examination. It is to be remembered, that the specimen examined by Mr A. had been detached from the animal.

By this time the chyle had disappeared from numerous vessels, and in many instances they were not easily detected. In the second specimen of this animal, dissected lately by me, though more than eighty large lacteals proceeding from the intestines, to the mesenteric gland, were filled with quicksilver, not a globule could be detected, either in the veins or in any vessel or *vas efferens*, which could be traced to the thoracic duct. * The reason of this I imagine to be, that the radicles of the *vasa efferentia* are very small, and that these vessels do not collect into one or two large branches, in the manner of most other animals, but proceed directly to the next plexus of absorbents, which in the dolphin supply the place of the *receptaculum chyli.* With all my efforts, and after the most careful dissection, I could only trace distinctly two other *vasa efferentia*, proceeding directly from one of the mesenteric glands, into a branch continuous with the thoracic ducts. Moreover it is to be observed, that the mesenteric glands in the porpoise are remarkably firm. I cut open several of them, and found the injected quicksilver uniformly contained in their proper vessels, whose very thin tunics formed a striking contrast with the surrounding firm substance, of which the gland is chiefly composed.

Without meaning to deny that there are certain grounds for supposing, that in certain animals, the chyle may follow a different route in its way to the general circulation, I may, in the mean time affirm, that we cannot infer, from the passage of quicksilver into the veins of dead animals, after traversing the *vasa inferentia* and mesenteric glands, that the chyle in living vessels adopts the same route to reach the general mass of the blood. †

3.

In my last letter to you, I briefly recounted the circumstances which led me to inquire into the anatomical distribution of the lacteals in cetaceous animals; and I detailed the results (somewhat unsatisfactory) of two extremely laborious dissections. In neither of these had I seen the *vasa efferentia chylifera ;* and being at the same time aware, that their existence, so far as I know, had been merely presumed by preceding anatomists, but had not been distinctly demonstrated, I felt disposed to think that there might be something in the anatomy of these animals, not very well understood. Some of the arguments for and against the doctrine of absorption of the chyle by veins I have stated in the letter immediately preceding this.

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* It is to be remarked here, that *only those vessels were filled with quicksilver which evidently contained chyle.*

† Mr Abernethy remarks, in his admirable Memoir already so often quoted, that "the ready communication of these bags (the mesenteric glands) with the veins of the whale, induced me to examine whether I could ascertain any thing similar in other animals. Air impelled into the lymphatic glands, however, seldom gets into the veins; sometimes indeed veins are injected from their glands, but when this has occurred to me, I have observed an absorbent arising from the gland, and terminating in the adjacent vein."

The result of the inquiry into the lacteals of the porpoise being thus by no means satisfactory to me, I availed myself of the first opportunity of again resuming it, and of conducting the dissection, and injection of the lacteals, somewhat differently. Having therefore obtained a third specimen of the porpoise,* my first care was to inject with quicksilver a considerable number of the lacteals high on the mesentery, and nearest to the commencement of the thoracic duct; the whole mass of the intestines was laid over to the left side, and that no impediment might be offered to the course of the quicksilver, a circumstance I neglected in my former dissections, I carefully dissected off the portion of the lower cava and aorta, which lay immediately over the commencing roots of the thoracic duct. At this stage of the dissection, I observed numerous vessels filled with chyle proceeding from the direction of the mesenteric glands towards the thoracic duct, and terminating in it; I felt convinced that these vessels were the true *vasa efferentia*, but unwilling to rest a fact of so much importance on the bare inspection of vessels, at all times deceptive in their appearance, I proceeded to fill about a dozen of the lacteals (the *afferentia*) with quicksilver, and shortly was much pleased to observe that the quicksilver, after traversing the glands, flowed along the *vasa efferentia*, mixed with the chyle, and passed directly into the thoracic duct. The number of the *vasa efferentia* so filled, might be about six, and the quantity of quicksilver conveyed by them into the thoracic duct was such as to endanger its rupture. I next distended the thoracic duct with air, and traced it to its entrance into the veins; the valves of this vessel were as numerous as I have ever seen them, and the tunics of the duct were stronger than I expected them to be.

I ought further to mention, that the column of quicksilver used was not more than nine or ten inches in depth; yet such was the facility with which the metal traversed the glands, and penetrated to the thoracic duct, that in order to secure the preparation, and retain the fluid metal in the *vasa afferentia*, I was obliged to place a tight ligature immediately outside these glands; previous to doing this, the vessels (the *vasa afferentia*) had been twice filled with quicksilver, which had as often disappeared. So many of the *efferentia* (which are very numerous) had been cut, that I found it impracticable to arrange the preparation in any other way. Moreover, I have no doubt that similar results may at all times be commanded by a little care and patience on the part of the dissector, and by attention to those circumstances which rendered my first dissections unsatisfactory.

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* The animal was about five feet in length, and proved to be a pregnant female.

We cannot, I presume, now hesitate in affirming, that the anatomy of the seal and of the porpoise do not furnish any argument against the long established doctrine of the transmission of the chyle, the nutrient fluid of the body, to the general mass of blood, by a peculiar system of vessels, consisting of one set of vessels which convey the chyle to the mesenteric glands; and of another set of a precisely similar structure, only somewhat larger, conveying the chyle more or less altered from the glands to the thoracic duct, and thence into the veins; and it seems proved that there is no other route by which the chyle passes into the blood, or at least that no necessity arising out of peculiarity in structure has yet been shown in any animal, for its adopting a different course. The facility with which the metal reached the *vasa efferentia*, makes it probable that a structure similar to what exists in the whale, as described by Mr Abernethy, will also be found in the porpoise; that is, that certain lacteals pass directly through the glands, maintaining a comparatively large calibre throughout; but I mention this merely as a conjecture. It will afford me much pleasure, should the result of this inquiry meet with your approbation.

Edinburgh, May 1824.

VII.

Observations on the Employment of Blood-letting in the Cure of Disease. By James Bonnar, Surgeon, Auchtermuchty.

The use of blood-letting as a remedy in disease, from the frequency with which it is employed, as well as from the facility of its application, becomes a subject of important investigation to every medical practitioner. It is important in regard to the distant and more indeterminate changes produced in the animal economy, by a frequent or extensive application of it, as well as in its more immediate effects on the system, in arresting those morbid operations which constitute a state of disease.

It were idle to attempt to deny the absolute necessity of blood-letting as a remedy in many cases of disease, but there are also every day to be met with in ordinary practice, cases in which its employment is a matter of choice, and in which other remedies may be equally efficacious in restoring the patient to health, and at a less expense to the general system. Every practitioner must be aware, that many acute cases occur which are referable to no specific nosological arrangement, and in the treatment of which the choice of remedies must altogether depend on the discretion of the physician, from the impossibility of laying down any absolute rules, which can be applicable to every individual case of disease. Cases are every day met with, where acute symptoms prevail in a habit of body chronically disposed, or perhaps actually labouring under some specific disease, in which blood-letting to any considerable extent is altogether precluded, or at least strongly contraindicated. In such cases, the acute symptoms may be so severe as to point out the absolute necessity for the use of strong measures, yet the extent to which it may be proper to carry them becomes a question of considerable nicety. The anomalous nature of diseases depends, in every individual case, on so many different circumstances, that any attempt at an enumeration of them would be needless. But a general character may prevail, pointing out a specific practice in the treatment of such cases. It is thus that much will depend on the prejudice or preconceived opinions of the practitioner, so that any attempt to ascertain the relative value of such a remedy as blood-letting is likely to be useful.

In every attack of acute disease, there evidently takes place an immediate attempt in the natural powers of life, to effect a removal of the existing evil, by means of a series of morbid actions. If those salutary operations do not interfere with the due performance of the functions of the animal economy, so far as to endanger life, a restoration to health is the natural result. But the means which nature employs is invariably productive of a certain degree of debility, proportionate to the extent of the existing disease, and the corresponding extent of those morbid means necessary to be called into action, in order to restore the patient to a state of health.

The duty of the physician is to assist nature in her salutary operations, and often to direct her in the choice of the means. The spontaneous efforts of the system, however, are often productive of the most imminent danger, and render the interference of the physician absolutely necessary, so far as to check those efforts and to substitute measures often of a very different kind. And laying aside the consideration of the danger incurred by leaving the case to nature, if the substitution of an artificial remedy be productive of a less degree of debility, or if by such interference the cure can be accomplished at less expense to the system, a great deal will be gained. It is thus that so potent a remedy as blood-letting becomes of inestimable value in the practice of medicine. It hence becomes a question of great importance in every case of disease, how far it is proper to interfere in the wholesome efforts of the system, and to substitute an unnatural remedy, such as the one in question, and to what exact length it may be proper to carry our interference. Much must depend in every individual case on the discretion of the medical attendant. These remarks do not apply to those cases so decidedly inflammatory, in which blood-letting as a remedy becomes a *sine qua non*, but to cases of daily occurrence, which exhibit no decided characteristics, but which, from their frequency, are highly important.

In this variable climate, a great proportion of the acute diseases met with in ordinary practice are cases of general excitement of the vascular system, attended by some local determination, producing what is called fever and inflammation; which determination may be in so slight a degree, as to warrant no specific arrangement of the case. I am also convinced that the larger number of those chronic diseases which afflict the human body owe their origin to an inflammatory condition, and are, in their first stages, diseases of excitement in the organs more particularly affected, attended by a general increased action of the heart and arteries, but which may for a considerable period be unattended by pain and other concomitants of inflammation; or those symptoms may exist in so slight a degree, as to lull the suspicion of the patient, and to elude the detection of the medical attendant. It is thus that many complaints are so far advanced in their progress, as to be beyond the reach of an effective application of medicine, before assistance be called; for it is well known that a morbidly increased action cannot continue in any texture of the body for any length of time, without at last destroying or in some measure changing the organization of the parts included in the morbid action; and consequently destroying or changing the healthy functions of the organ affected.

The early and decisive use of blood-letting in all inflammatory diseases is universally allowed to be of the greatest importance. The loss of a given quantity of blood in the beginning of an attack is of more utility in stopping the progress of the disease than the loss of a much larger quantity at a later period. To the reduction of strength occasioned by the larger depletion rendered necessary by procrastinating, we have to add the debility consequent to the longer continuance of the disease, laying aside the consideration of the danger to life resulting from an inert practice.

In cases where life is not immediately at stake from the limited extent to which morbidly increased action prevails, a choice is left to the practitioner to substitute other remedies, such as purgatives, diaphoretics, &c. But in every individual case, considerable discrimination is necessary; and I am inclined to recommend a more general adoption of sanguineous depletion in the beginning of diseases than is usually practised. In the beginning of a great number of those complaints resulting from the application of cold, a moderate bleeding is, in my opinion, to be preferred to purgatives, &c., when used with a view of lessening the quantity of the circulating fluids. Purgatives are excellent auxiliary remedies, and are of the greatest service when used to correct any derangement in the *primæ viæ*, which might prove a source of irritation, and thus keep up the morbid action. But I cannot help considering a dependence upon them as a chief means of cure in diseases in which the action of the heart and arteries is much excited above the natural standard, to be a practice pregnant with the most imminent danger.

Inflammation in some textures of the body, is so obscure in the beginning, so insidious in its progress, and so rapid in its termination, that in all cases in which the physician hesitates respecting the necessity of bleeding, it is, in my opinion, a wise plan, to deprive the patient of the benefit of the doubt, and immediately to proceed to the operation. The insidious nature of inflammatory action is in no disease better exemplified than in enteritis. How often does it happen that a practitioner meets with a case which he conceives to be one of simple obstruction *of the bowels*, proceeding from previous neglect on the part of the patient, or perhaps he pronounces the disease to be colic, occasioned by the application of cold, or the presence of some improper article of food taken a short time before? In neither of these cases is the patient affected with rigors, the pulse is not much above the natural standard, or perhaps it is weaker than natural, vomiting does not prevail to any considerable extent, the tongue is only slightly furred, not dark to appearance, and only such as might occur in any ordinary case of sickness, and to a superficial observer the abdominal pain is not much increased by pressure. He treats the case by a frequent exhibition of purgatives and enemas; fomentations, and perhaps the warm bath is resorted to, without much benefit; still he assures the relatives that there is no danger.

In the course of two or three days at the utmost, abdominal swelling, hiccup, cold clammy sweats, occur; in desperation he flies for the first time to the lancet, which now only serves to hurry on the fatal catastrophe. At length the black vomit comes on, and puts the nature of the case beyond a doubt, and the patient expires. It can only be known to the truly practical physician with what promptness it is many times necessary to have recourse to blood-letting, on the very first accession of disease. In some instances the continuance of increased vascular action for a very short period places the patient beyond the reach of medicine :—hence the great importance of decision in all those cases where there is the slightest suspicion of inflammation existing. A hesitating practitioner, who takes a few hours only to make up his mind respecting the course he is to pursue in the treatment of a doubtful disease, may often thus doom his patient to an irretrievable fate. I hold it as a maxim, that it is more culpable in a physician to lose his patient by neglect or indecision, when it might be in his power to save him, than it is in one who hurries on the fatal termination by the use of desperate measures in a desperate disease. In these remarks, I keep entirely out of view those innumerable instances of inflammatory disease of every day occurrence, and in the treatment of which the merest tyro would be ashamed to hesitate.

There is a period in every disease wherein blood-letting has been employed, when the medical attendant asks himself how far it may be proper to stop in the use of the remedy, and to trust for the completion of the cure to the efforts of nature and the use of auxiliary measures, or to carry it still farther. An unnecessary loss of blood weakens the patient, and, it is supposed, induces a state of positive disease. But, in my opinion, stopping short of the quantity necessary to be lost is by far the more dangerous side on which to err. The loss of a quantity of blood more than may be exactly necessary for subduing the morbid increased action is in a healthy subject very soon made up. The assimilating powers of the system provide, in a wonderful manner, to supply the deficiency, first in point of quantity, and then in quality, whereas a few ounces less than is necessary may render the loss of the quantity already taken away perfectly useless. However, a facility in deciding on the exact point where we ought to stop in the use of so powerful a remedy as blood-letting in acute disease is much to be desired, and it is that nice tact which cannot be inculcated by any rules, and is only to be acquired from actual practice and attentive observation, which stamps the superiority of one physician over another, and by some is never to be acquired.

In many inflammatory cases, it is truly astonishing to witness the great length to which it is sometimes necessary to carry blood-letting before any evident impression be made upon the disease; and when its use is followed by a favourable termination of the case, the degree of debility consequent to so great a loss of blood is much less than could *a priori* have been expected. The effects of blood-letting in subduing the inflammatory symptoms are by no means in proportion to the extent to which it is carried, nor does relief at all keep pace with the quantity of blood detracted. A patient may be two, three, or even four times bled without the slightest abatement of any of the inflammatory symptoms, or even while he is undergoing the operation of depletion, there will often take place in fact an aggravation of all the symptoms of the disease, and it will not be till after the loss of a given quantity of blood, that any feeling of relief whatever will be experienced.

Mrs P., aged 45, who had had a large family, was some time ago, without being previously indisposed, suddenly seized with severe spasms in the stomach. There was at first no increase of pain on pressure; pulse nearly natural; mouth parched, but no thirst. In the course of a very few hours, the spasmodic pain extended over the whole abdomen, which very soon became tender on pressure, particularly opposite to the stomach. Tongue streaked with a dark brown fur; she had alternate chills and heats. There could now be no doubt of the nature of the attack, and no hesitation respecting the plan of treatment to be pursued. The bleeding was repeated in a few hours, notwithstanding which, and the unremitting use of auxiliary remedies, the symptoms of the disease steadily got worse. The greater part of the abdominal contents seemed to partake of the inflammatory action. The stomach rejected her drinks almost as soon as taken, and she had an almost constant eructation of hot watery fluid from the stomach. Her bowels were obstinately bound up; the uterus was involved in the increased action. It seemed to be hard, and about the size of a large fist, and very tender to the touch. The pulse rose in frequency. Aided by the advice of Dr Kellie of Leith, whose professional character stands deservedly high in the estimation of his brethren, the farther detraction of blood was persevered in, and the application of other remedies was at the same time unremittingly used, but without the slightest mitigation of a single symptom. In consequence of my having been called at the beginning of the attack, and from the early application of antiphlogistic remedies, the disease did not make that alarming progress which so often happens in abdominal inflammation. The same measures were persevered in for some days, without any appearance or even hope of subduing the disease, till one hundred and thirty ounces of blood were taken away, besides the loss sustained by the application of twenty leeches. At length abdominal swelling to a considerable extent occurred. The teeth and lips were covered with sordes; the tongue was deeply furred with blackish matter. No alvine evacuation had been procured, except in the smallest quantity, by the incessant administration of purgative enemas. In a state of the greatest anxiety for the safety of my patient, I resolved to risk one more decisive bleeding. There had as yet occurred no perceptible sinking. I had been bred in an excellent school, under some of the ablest naval surgeons, in a warmer climate, where prompt and decisive medical practice is of so much importance, and had often witnessed the salutary effects of large bleedings in inflammatory diseases. I made a large orifice, and at once took away forty ounces on the sixth day of the disease. My patient fainted for the first time, and so completely, that for a few minutes, on considering the large quantity of blood previously lost, I was afraid that my patient would slip through my fingers. From that moment the disease was overcome, and my patient had afterwards no return of inflammatory excitement, rendering any further detraction of blood necessary. In this case I may remark, that from the very steady manner in which all the symptoms got worse during the first six days, notwithstanding the unremitting use of powerful antiphlogistic remedies, we have every reason to believe, that had it not been for the last decisive bleeding, the result would in all probability have been fatal. It appears that the quantity of blood taken away in a dangerous inflammatory disease, though very large, is still insufficient to arrest the progress of the morbid action, until some powerful effect be produced upon the nervous system at large.

In the case just related, the patient lost, in the course of six days, upwards of *one hundred* and *seventy* ounces of blood by measure, exclusive of the quantity taken away by twenty leeches. The debility, as might have been expected, was very considerable, but by no means greater than that which often results from a ten or twelve days continuance of a smart fever. Nor did there take place any œdematous swellings which systematic authors would lead us to expect as the frequent consequences of large bleedings. The catamenia returned in due course, within a week of the last bleeding; but the discharge was so void of colour, as scarcely to stain the linen. After blood-letting, the assimilating powers of the system seem to make up very soon the deficiency in point of quantity; but the supply of red particles is the work of a much longer period.

I should suppose that the instances are exceedingly rare in which death ensues as the immediate result of excess in blood-letting, but I can easily conceive, that, in a habit of body where particular idiosyncracy of constitution may render the detraction of an excessive quantity rather a critical practice, and a suspension of the action of the heart and arteries may be so complete, the renewal of their functions may become a matter of considerable difficulty. It often happens, in cases of internal inflammations, that disorganization has already taken place, but without evincing itself by any evident symptoms so as to render it cognizable by the practitioner, who, in his anxiety for his patient, has recourse to blood-letting, after all possibility of its doing good is past, and in a very short period afterwards the patient dies, not from the loss of blood, but as the natural event of the disease; or, that the vital principle is so much exhausted by the continuance of the febrile excitement, that blood-letting hurries on the fatal termination. In such cases, the relatives and attendants are apt, however unjustly, to reprehend the practice of the medical attendant, even to the aspersion of his character. In justice to himself, it thus becomes a duty of every physician, more particularly in his practice in the uneducated class of society, to watch attentively the accession of mortal symptoms, before entering on a decisive practice in such cases as active internal inflammations, which are so rapid in their progress, and oftentimes so unforeseen in their termination.

In those diseases in which blood-letting is applicable as a remedy, but its employment not absolutely necessary, and in which the having recourse to it is a matter of choice with the physician, I am decidedly of opinion that a greater degree of debility will be produced by leaving the case to the efforts of nature, aided by purgatives and a cooling regimen, than by the detraction of blood during the first days of the attack, and cutting short the disease by active measures, although both plans of treatment will be equally followed ultimately by a restoration to health. For example, in an ordinary attack of catarrh, in which there is no particular determination to any organ, but where there is only a degree of general febrile excitement, some days confinement to bed, and the exhibition of a few doses of purgatives, will be followed by a greater reduction of strength than would have resulted from the employment of one or two full bleedings.

I have already remarked, that morbidly increased action cannot possibly exist, even for a short period, and to an inconsiderable degree, in any texture of the body, without the risk of its producing a change in such texture, and in this manner laying the foundation of chronic ailments. Hence the great importance of cutting short, in every case, the stage of excitement. This is well exemplified in the case of rheumatism, the first stage of which is generally characterized by the presence of febrile excitement. And it is found, that the violence and the period of continuance of the acute symptoms will determine very much the severity of the subsequent chronic affection, and that the use of those active measures, which are calculated to subdue the existing inflammation which prevails in the beginning of the attack, diminishes in proportion the violence and the period of continuance of the chronic sequelæ.

P. Bruce, a middle-sized man, in the prime of life, was some time ago seized with an attack of acute rheumatism. The whole muscular system was so much affected that he could not be moved, and scarcely touched, without suffering extreme torture, and of himself he was so helpless that he required to be fed and lifted like a child. He had violent fever, and other concomitants of inflammation. None of the viscera were affected. The most rigid antiphlogistic remedies were resorted to, and he was in the course of four or five days blooded from the arm, six different times before the violence of the attack gave way. Each time as the blood flowed, he expressed himself relieved. By the unremitting use of auxiliary means, the muscular pains went completely off, and in the course of a few weeks he was restored to health, the attack leaving no chronic affection whatever behind it.

About the same time, John Gardiner, a strong young man, about twenty-six years of age, was seized in a manner exactly similar. He was once largely blooded, which produced syncope; but I could not prevail on him to submit again to the use of the lancet. The cooling regimen, purgatives, diaphoretics, &c. were afterwards used throughout the course of the disease. He was confined to bed in a helpless condition for a considerable time, and after the stage of excitement terminated, the case ended in a violent species of *sciatica*, which rendered him lame for many months.

In both, the violence of the attacks was much the same. At the termination of the acute stages, the debility in the first case was much less than in the second, notwithstanding the loss of nearly a hundred ounces of blood. P. Bruce was able at the end of a few weeks to resume his usual employment, while Gardiner was unable to attend to his business for a period of six months.

I am inclined to recommend the use of moderate depletion in rheumatic affection farther than is generally practised. I have often witnessed the beneficial effects of it, with the debilitated, and those a little advanced in life, when used with discretion in the beginning of rheumatic attacks, even when the febrile excitement was by no means great, and in cases in which it was not strongly contraindicated. It is much less debilitating, than the usual practice of sweating and blistering. However, I must except those in whom chronic rheumatism is habitual.

The debility resulting from blood-letting I rather apprehend to be overrated even by medical men. Every practitioner must be aware, that in all cases of overloaded circulation, the immediate effect of blood-letting is stimulant. Whether or not in febrile excitement the muscular debility which is so generally present, proceeds from the pressure of the blood upon the nervous fibrillæ, which are in contact throughout the whole system with the vascular ramifications, and which pressure is occasioned by the increased action of the heart and consequent increased impetus of the blood; still the fact is evident, that blood-letting in fever gives immediate strength to the patient, even when neither sensorium nor any of the viscera are particularly affected. This I have often witnessed in continued fever, where the patient required the help of others to enable him to get out of bed in order to undergo the operation, and immediately afterwards was able to return to it without the least assistance.

The facility with which the assimilating powers replenish the vascular system after excessive blood-letting, is truly astonishing. I have already observed, that the deficiency is in the first place made up in point of quantity, and afterwards the blood is improved in quality, in a very gradual manner. The stimulus which intestinal absorption receives from the use of large evacuations, does not stop in its operation after establishing the natural degree of fulness of the vessels which existed at the beginning of the case, but goes on beyond the healthy point, and creates a condition of plethora or of overcharged circulation. Hence the pernicious tendency of too great an anxiety to improve the strength, by the use of stimulants and tonics after an attack of acute disease. In all those diseases proceeding from a plethoric condition of the system, and which require the detraction of blood in greater or less quantity, in order to avoid immediate danger, a spare regimen cannot be too strongly inculcated during the period of convalescence. For from the cause assigned above, the necessity in such cases is but too apt to become habitual, and the more frequently it is had recourse to at distant periods, the necessity of repetition becomes more and more, and the intervals shorter and shorter, till at last a periodical loss of blood becomes absolutely necessary to the existence of the individual.

A most extraordinary instance of frequent losses of blood, and of the reproductive power of the animal system in supplying the deficiencies created, occurred some years ago in my practice. James Wishart, carpenter, a strong active man of 30 years of age, of a healthy constitution, was, in the summer of 1808, employed in cutting wood during a very hot day. After being much warmed with labour, he was exposed to a very heavy rain. He was seized, in consequence, with a violent attack of catarrh, for which nothing particular was done by way of medicine. He continued ill for a number of weeks, when the case ended in an abscess in the lungs. About two months from the beginning of the attack, the blood burst from his lungs to an alarming extent. He was now blooded from the arm for the first time, which had the effect of stopping the hemoptysis. The cough and expectoration of pus continued unabated, and he had every symptom of hectic fever. In the course of ten or twelve days afterwards, the discharge of blood recurred. The hemorrhage must have proceeded from a vessel of considerable size, as the quantity lost in a very few minutes was very large, sometimes amounting to four pounds and upwards. Bleeding from the arm was again had recourse to, and had the effect of stopping the discharge from the lungs. He then felt tolerably well, and free from any bloody expectoration, until the circulating mass was increased, and the vascular system became again overcharged, when the same kind of hemorrhagic attack took place, and continued till the system relieved itself, or until he was largely blooded from the arm. Before the discharge from the lungs abated, it was sometimes necessary to use the lancet two or three times within the space of sixteen or twenty hours. Before the attack, he felt the whole system overloaded, particularly about the head; and his breathing was more oppressed. On watching his situation, he sometimes applied for relief, and was largely blooded from a vein, which had the effect of preventing the expected attack. He thus continued to be liable to these attacks, at intervals of from ten to twenty days, for the period of seven years and a half, during which time he was blooded from the arm upwards of *three hundred* times. Towards the latter part of his illness, he became my patient; and notwithstanding every attempt to check the tendency to the formation of blood by spare diet, &c. and to moderate the action of the heart and arteries by the use of digitalis, I did not even succeed in lengthening the intervals between the attacks. During the period of his illness, the phthisical disease made no progress. The purulent expectoration was at all times very great; but he at no time laboured under great debility, excepting occasionally what might naturally be expected from the loss of a large quantity of blood. He was, for the most part, able to go about, and even to do a little work. At length, without any evident cause excepting perhaps his working more regularly, the pulmonic ulcer healed. He no longer required venesection. Every vestige of disease disappeared, and he is at this moment in perfect health. He was then about thirty-seven years of age. Before getting well, the intervals between the hemorrhagic attacks became longer and longer. At no time during his long illness had he any tendency to œdematous swellings, which may be perhaps considered as remarkable; for the quantity of blood lost, in a short period of time, was occasionally very great;—six or seven pounds within twelve hours. I never before knew of a case in which blood-letting was so often repeated, and where the disposition to the formation of blood went so completely off without the accession of some other disease which exhausted the system, or the occurrence of a discharge of blood from a different part of the body, generally from the bowels, more gradual and more constant in its operation.

Systematic authors would lead us to believe, that dropsy and anasarcous swellings are to be dreaded as frequent consequences, resulting from excessive depletion; but I am inclined to think, that the risk has been much overrated, and that in those cases in which such things have taken place, they have oftener followed as the consequences of those diseases for which it was found necessary to use large bleedings, than as the effects of the remedies employed.

When serous effusion takes place in inflammatory affections of the brain and lungs, I apprehend it invariably happens as the immediate consequence of the increased vascular action of the parts affected, which constitutes the disease. And in no case was I ever deterred from using blood-letting when there was a prospect of doing good, from any dread of such occurrences taking place. I have often witnessed relief in breathing to follow a moderate bleeding in the last stage of pulmonary inflammation, even after an effusion of serum had to a certain degree taken place, which very shortly afterwards carried off the patient.

An inflammatory condition of a part naturally produces a laxity and an increase of diameter of the capillary vessels, which admit the passing off of a larger quantity of the serous part of the blood into the neighbouring cavities than what is natural, and more than what the corresponding absorbents are able to carry off; hence the occurrence of watery collections. Those means, therefore, which are calculated to subdue the inflammatory action, are the best adapted to prevent and to cure such dropsical affections. In this manner, we may account for the success attending the employment of blood-letting, in those dropsies resulting from the application of cold, in which I apprehend there always exists, in the cellular texture of the parts affected, an inflammatory state, which is the immediate cause of the disease. In like manner may we account for the occurrence of anasarcous swellings after scarlatina, and for the success of the treatment by depletion.

In those cases where the efforts of nature, by establishing a discharge sufficient to relieve the overcharged condition of the system, and thereby checking the local morbid affection, are proceeding in a salutary course, it is often a dangerous practice to interfere, by using so strong a measure as general blood-letting. For the outlet established by nature is generally nearer the seat of the disease, and is more powerful in its effects than any measure which we can employ by way of blood-letting, which can act only through the medium of the system at large.

Some time ago a gentleman was attacked with catarrh. The general fever was not great; pulse 82, and rather full. The chest appeared to be the part more particularly affected, but without the slightest degree of pain. Respiration was free and natural. On the second day when I first saw him, expectoration had come on, and was free and copious. I was averse to the use of strong measures, and declined taking more than 14 ounces of blood from the arm, and he was put under the strict cooling antiphlogistic regimen. Next day a physician saw him and bled him largely, and applied a large blister to the chest, which treatment had evidently the effect of forming a degree of congestion in the lungs. The pulse rose till it reached 125; the expectoration almost entirely ceased; the breathing became hurried and laborious, and the patient was placed in imminent danger. There was now no alternative but to persevere in the detraction of blood, and it was found necessary to carry it to the extent of 90 ounces, before the violence of the disease gave way. The patient ultimately got quite well. What is worth remarking, the expectoration did not return. In a case such as this, I cannot help considering such decided interference to be bad practice. The state of the pulse, and the total absence of pain, certainly pourtrayed no considerable degree of inflammation; and from the occurrence of a free expectoration, there was reason to hope for a favourable termination to the case. I am inclined to think, that in a case of pneumonia or catarrh, a single ounce of mucus expectorated, or the transfusion of the smallest quantity of blood from the vessels of the part affected, will do more towards the reduction of the disease, than the loss of forty times the quantity of blood from the system at large.

There are many cases in which blood-letting is not expedient as a remedy, although it appear to be pretty strongly indicated, arising from certain irregularities of the circulation, in which it would no doubt give relief to the present feelings of the patient, at the same time it would protract the continuance of the case, and lay the foundation of much future evil. In young female subjects with whom the menstrual discharge is not fully established, there often arise particular feelings and symptoms resembling those produced by a general plethoric condition of the system; and in fact there does exist in such cases a fulness of the vascular system, but not to the exact extent sufficient to enable nature to support health by the establishment of a discharge necessary to the due performance of the functions of the animal economy. That condition of the circulating system alluded to, which is preliminary to the full development of the female constitution, and which, to a certain degree, precedes every return of the catamenia, often exhibits symptoms very much resembling those of actual disease, but which are invariably relieved by the occurrence of the discharge. I am of opinion, that in such cases our interference, by substituting an artificial evacuation so powerful as blood-letting, is calculated to do much harm, by forcibly checking the salutary efforts of nature. In those cases, the unpleasant feelings proceed not from too great a fulness in the system generally, but rather from the current of the blood taking wrong determinations, producing a state in which it exists in larger quantity in one part than in another of the body, though at the same time the quantity generally may not much exceed, if at all, what is to be regarded as the healthy standard. Under such circumstances, the detraction of blood, by diverting the current from its proper channel, or, to speak more properly, by removing that degree of fulness or plethora which may be considered to be essentially necessary for the production of a salutary effect, and thereby preventing what is absolutely required for the due maintenance of health, will be productive of a state of actual disease. At that period of life when the menstrual discharge is about to be established in females, there is a peculiar susceptibility of the nervous system, which is much increased by any interference on our part by way of evacuations. And I have often had occasion to observe, that the necessity of large depletion at that period has entailed on the patients a liability to a train of hysterical affections, and which has never wholly left them. I am unable to explain why, at that time of life, the nervous constitution is more easily acted on by blood-letting; still the fact of its being so should make us cautious in having recourse to so powerful a measure, or indeed to any means whatever, calculated to interfere with the natural progress of uterine action. And moreover, the relief obtained by the use of powerful evacuants, from the uneasy symptoms occasioned by overloaded circulation, in a case of amenorrhœa, is temporary, and continues only till the next period of menstrual return, when the same feelings again recur; and if the same plan of treatment be a second time resorted to, a periodical necessity for blood-letting will be established, which afterwards will not be easily overcome, as every successive attack will more and more require the use of those depletory measures, which eventually will be found to undermine the general health, while, at the same time, the establishment of the natural discharge by the uterus will be placed farther and farther off.

Systematic authors point out the appearance of the blood already taken away as a criterion by which we are to be directed in our practice regarding the farther use of depletion in the cure of the disease. But it is in my opinion a very uncertain guide; for in all attacks of acute disease, those circumstances which give to the blood that appearance so well known under the name of the inflammatory coat, exist for some time after the force of the attack is broken, and the blood will not resume its natural appearance till some time after all necessity for farther depletion is past. The existence of the buffy coat upon the blood, whether as a proof of the presence of morbid action, or as it is found in particular states of the animal system, seems to depend entirely on an increased momentum and consequent increased temperature of the circulating mass, and perhaps also on some undefined change with regard to fluidity. I am much inclined to think, that the inflammatory coat will in no case be exhibited till an increased momentum is perceptible, and that no local inflammation will long exist in any part of the body without occasioning such increased momentum. The increased action in the heart and arteries in inflammation seems to depend on a nervous sympathy which extends throughout the ramifications of the whole sanguiferous system. No morbid increased action can be supposed to subside all at once, however powerful the means employed may be. Under such circumstances, then, the continued exhibition of the buffy coat is no proof of the unyielding nature of an attack of disease, or of its not having already given way to the means employed. I even believe, that in a violent inflammatory attack, the practitioner may continue to bleed his patient to the dregs before the blood resumed its natural appearance.

The very frequent occurrence of inflammatory diseases, of late years, is often the subject of remark; but I am by no means inclined to admit the truth of such a remark to the extent generally supposed. I have no doubt, however, that the changes which have taken place, of late years, in the manner of living among almost all classes of society, may have had the effect so far of making the prevalence greater of diseases of excitement now than in former times. But I am led to ascribe a much greater share of the belief, to the late improvements in medical nomenclature, and consequent improvement in the treatment of diseases. On looking back, not more than twelve or fourteen years ago, while I was in my apprenticeship, I recollect, that almost all acute cases which occurred, went under the general denomination of fevers. Inflammation was a term never heard of in this part of the country, excepting in the case of a wound, or some other accident. Bleeding was a remedy almost entirely proscribed in internal diseases. The mortality in all acute diseases was undoubtedly greater than at present; still a very great number got well under a constant exhibition of Peruvian bark and half a bottle of port-wine daily.

If the feelings of our patients were to be taken as a criterion of the propriety of our practice, we have ample encouragement to persevere in the employment of the depletory plan of treatment in many of those diseases in which blood-letting has of late been used. For, in epidemic fever, and the exanthemata, such is the relief experienced from a first bleeding, that I have in general not only found my patients willing to undergo it a second, or even a third time, if found necessary, but often they of themselves solicited a repetition of the remedy. For a long time after beginning practice in this part of the country, I found a strong prejudice to prevail against all powerful evacuations in all those diseases, which the people had been accustomed to call *fevers,* and also in all eruptive diseases, however violent the local affections might be. They submitted most willingly to blistering and sweating and purging. But these prejudices are now wearing fast away.

It cannot be denied, that the habits and modes of living which prevail in civilized society, are greatly calculated to increase the prevalence of diseases of excitement, as the certain consequences of pampered appetite and diminished activity in regard to exercise. A liability to disease depends in many cases more on the existing condition of the body favouring the production of such disease, than on the strength or violence of the stimulus, or more immediate exciting cause. So that we find that diseases of an inflammatory character may, in some individuals, be produced by one tenth part of the exciting cause which would be required to produce them in another. Hence, we observe that those persons whose systems are kept in a lower degree of tone, in consequence of their labouring for some time under some slight chronic ailment, such as rheumatism, dyspepsia, or even toothach, enjoy a particular immunity from attacks of inflammation, and even from prevailing epidemics. On the same principle, we observe that many persons, immediately before being attacked with a dangerous disease, enjoyed a more than ordinary degree of good health, during which period they contracted the plethoric condition, which perhaps was absolutely necessary to the occurrence of the disease. Causes productive of inflammation appear to act in a considerable degree through the medium of the nervous system, so that indulgence in living, the dissipation of late hours, and the neglect of a proper degree of exercise, by increasing the tone of the nerves, and by rendering them more irritable, contribute as much in this way to the production of disease, as by directly increasing the contents of the circulating vessels.

In the preceding remarks regarding blood-letting as a remedy in disease, I by no means intend to recommend it, to the exclusion of other and powerful auxiliaries. Purgatives and other measures are capable of fulfilling important indications in the cure of many inflammatory diseases, which blood-letting is by no means calculated to do. But I have endeavoured to ascertain the great importance of that powerful remedy in many diseases. And my remarks may do some good in removing those prejudices which still actuate the minds of some medical men in their ordinary practice.

Auchtermuchty, Nov. 22d, 1823.

VIII.

Observations on Amputation. By THOMAS CREASER, M. D. Cheltenham, late Surgeon to the Bath City Infirmary. Communicated by J. FOSBROKE, Surgeon, Cheltenham.

MANY years since, when engaged in the practice of surgery at Bath, and in the execution of my duties as surgeon to the Bath City Infirmary, the necessity of performing amputation frequently occurred to me. At that time (viz. ann. 1802) the mode, as improved by Mr Alanson, was in general adoption; when my lamented friend, Dr Clement Archer, staff-surgeon to the Irish forces, and state-surgeon to the Lord Lieutenant's household, communicated to me a method of amputating with which I was previously unacquainted. On applying it practically, its superiority, to my conviction, was greatly manifest. The invention of this mode was related to me as belonging to Mr O'Halloran, surgeon of Limerick, who was previously distinguished by his first adoption of the mode of operating by a single flap near the ancle-joint. Whether Mr O'Halloran was really the inventor I cannot precisely ascertain from Dr Archer's account of it, as I believe the leading principles of this operation were noticed in the "Memoires de l'Academie de Chirurgie" of Paris. In Mr S. Cooper's "First Lines of the Practice of Surgery,"* he quotes the following observations from Dessault. "When in consequence of the flesh being diseased or injured on one side, the flap must be entirely formed on the other, it will be necessary to save more skin and muscle in the latter situation, than would be requisite, if the surgeon were to have it in his power to form *two flaps* for covering the bone. In performing *the latter operation* in the thigh, Dessault used to grasp the flesh in its inner side with his left hand, and to pass a strait narrow sharp pointed knife, with its edge turned towards the knee, through the soft parts, thus pushing it on from the fore part of the thigh until the point comes out at the back of the limb. The incision was then extended obliquely downwards so as to make a flap about four inches in length, comprehending part of the cruralis, vastus internus, femoral artery and vein, the anterior crural nerves, triceps, sartorius, gracilis and semi-membranosus and semi-tendinosus muscles. This first flap was reflected, and the femoral artery with the trunk of the profunda tied. The external flap was next formed in a similar way, and consisted of the rest of the cruralis, rectus, vastus externus and biceps. The two flaps were then held back, and the bone sawn through as high as possible, and the other bleeding vessels secured.

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* Last Edition, "Œuvres Chirurg. de Dessault."

"Lastly, the flaps brought down so as to meet in a perpendicular line, to cover the end of the bones."—*Cooper's First Lines of Surgery, page* .

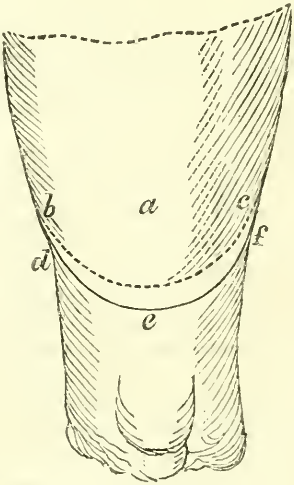
I do not mean, in the smallest degree, to infer that Messrs Liston and Syme derived their mode of performing amputation from the remarks of Mr Dessault; but as a mode executed on similar principles, appears to have been known to different surgeons, I think that the public are essentially obliged to Messrs Liston and Syme for having given it *publicity*, and for having so completely demonstrated its great superiority over all previous modes of performing these operations either before or since the promulgation of Mr Alanson's method.

The mode of forming two flaps in the amputation of the thigh, is already described by the copy from Mr Dessault's report of it, in his "Œuvres Chirurgicales." This mode is still more *completely defined*, its advantages demonstrated, and its present and ultimate superiorities so entirely proved by Messrs Liston and Syme, and these conclusions so entirely coincide with my own experience, that I shall feel disappointed if this mode is not speedily adopted by every hospital, metropolitan or provincial, in the United Empire.

I shall now describe briefly the mode of performing the amputation of the thigh, and upper arm, and (with modifications) of the same mode, below the knee and elbow-joints, which I have already alluded to, and which differs in some material respects from the operation described by Dessault, and lately so ably executed and demonstrated by Mr Liston and Mr Syme.

In this method, the incisions through the skin are to be made from *without inwards*, being opposite to the direction employed in the other way. When the incisions are made vertically by cutting through the muscular substance and skin from *within outwards* in two oblique directions, having first passed the knife vertically through the limb, there is certainly an addition of the whole of the pain produced by the passage of the knife vertically. Besides, it has appeared to me, that the incisions both through the skin and muscles are performed with more neatness and facility from without inwards, than *vice versa.*

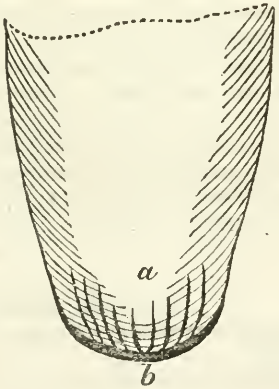
Waiving any directions as to the application of the tourniquet, or the dispensing with it (though I think the latter preferable), the integuments of the limb are to be retracted backwards by the hands of an assistant, and an incision through these, on the upper side, is to be made in a *semi-elliptic form.* The same incision is to be made on the lower or inferior side of the limb, so as to join the first incision at corresponding acute angles. The integuments being then retracted together, so as to leave an open space in the divided skin of about half an inch opposite, any adhesions between the cellular substance and fascia are to be slightly divided by the same knife with which the incisions were made, and by its convex extremity. Another incision is then to be made from the *edges* of the retracted integuments, on the upper side, and obliquely inwards, and upwards to the bone, and to the spot where it is intended to saw through it, and which may be calculated as lying in a right horizontal line with the junction of the angles of the superior and inferior flaps. The smallest portion of the periosteum of the bone may be separated by the same knife which effected the incisions, and will be sufficient to facilitate the cutting of the saw. The annexed sketch is intended to represent this mode of operation, and will illustrate it better than description.



a, The spot where the bone is to be sawed, being in a line with the junction of the angles of the incisions *b* and *c*.

d, The space between this and the dotted line above, denotes the space through which it is necessary to retract the integuments, before cutting through the muscles.

The incisions through the muscles are to commence exactly close to the retracted integuments at the dotted edge, inwards to the bone, and obliquely upwards. If these incisions are accurately made, and the same through the muscular parts executed in the direction described, they will then exhibit two nearly hemispherical sides. These may be held back by linen retractors, or by the hands of an assistant. In this way, an open space is afforded for the taking up of the arteries, instead of the assistant being embarrassed by acting in a cylindrical cavity, as in the older modes of amputating. The hemispherical cuts allow also of the most complete and well-formed approximation of the incisions. They exhibit a semi-elliptic form of the stump externally, leaving an ample covering of the bone, both by cellular and muscular substance, in the situation where this covering is precisely required. The form of the stump, when the operation is accurately done, is such as here sketched. It will be found to differ from the stump formed by the previous operations, in respect to amplitude of covering of the bone, and of symmetry of form; the projecting angular corners produced by the former modes not being produced in this way, and the obstacles which they form to union avoided.



a, Situation in which the bone should be sawed.

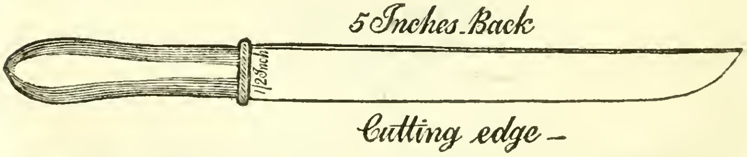
b, Covering of the stump above the bone.

The operation above the elbow-joint of the arm is to be executed exactly in a similar mode, *i. e.* by forming two horizontal flaps.

When amputation is to be performed below the knee-joint, and so high up as to leave the stump formed by the knee-joint bent, although here the advantages in this mode of operating are not so great as in the situations above the knee and ankle-joints; yet, if the incisions through the integuments and muscles are made horizontally with two flaps, one inferior, the other superior, in the semi-elliptical form before described, the union of the parts will be more rapid, and the stump better formed.

When amputation is to be performed low down in the leg, so as to preserve the use of the knee-joint, and to affix a short artificial leg, the mode described is peculiarly advantageous. The method described by Mr Hey, and so accurately defined in his Essays, may be more simply and easily effected by an adoption of the principle laid down, *i. e.* by making two horizontal flaps, one of the integuments on the upper side of the leg, about four inches above the ankle-joint, the other through the integuments and solæus muscle, rather larger than the superior one.

Every surgeon feels the importance of the instrument employed to execute his intentions. The knife, long since and repeatedly used by myself, does not exceed five inches in length beyond the handle, and half an inch in width of blade. It is much lighter, shorter, and narrower, than the amputation knives formerly employed, and which resemble more those used by butchers to cut inanimate matter, than to operate on the living. Mr Dessault's knife is described as pointed, as that of Messrs Liston and Syme must be, to penetrate vertically through a limb. Not having to cut through the parts by the point, but to make an incision from without inwards, my knife does not require to be pointed, but is formed with a convexity on the cutting edge, at the extremity of the blade. This form will be found to be excellently adapted to the formation of the semi-elliptic incisions, and is of the annexed shape and dimensions.



Mr Sully, M. D., surgeon to the Wiveliscombe Dispensary, whose talents are the most distinguished, not only in his own vicinity, but more extensively, was, many years since, made acquainted with the double flap operation, and has performed it often with uniform success. I do not know of any other surgeon in England, who has adopted it according to this mode.

Cheltenham, Feb. 20.

IX.

Case of Puerperal Fever, successfully treated by Oil of Turpentine. By HENRY PAYNE, M.D. Nottingham.

I send you the particulars of a case of puerperal fever, which has just fallen under my care, with the view of establishing a different method of treating that disease, from that which, it appears, is still employed by many practitioners both in Edinburgh and London. The practice to which I allude, as employed in these cities, is blood-letting; but in justice to the Edinburgh physicians, it ought to be mentioned, that the lancet is only recommended by them to be used in the earliest states of the disease, immediately after its commencement, and on no account after it has continued many hours. Whereas, according to the statement of a case published in the 11th Number of the *London* Medico-Chirurgical Review, that remedy is *there* repeatedly employed, even until after there is a prospect of a fatal termination of the disease.

The case of puerperal fever, of which I am now going to give you an account, occurred in a married lady living in this town, on Sunday the 19th of January 1823, and I was requested to visit her in the afternoon of the 20th. At half-past 4, *p. m.*, I found her complaining of pain in the lower part of the abdomen, but most acute on the left side, nearly as low as the groin. The pain was much increased on pressure. There were much headach, urgent thirst, an intolerance of light, and great disturbance produced by the slightest noise. The pulse was 140, full and strong. There had been, just before my visit, some nausea and vomiting. I could not find that there had been any rigor, but a coldness of the lower extremities, creeping up the back, had been complained of. The milk appeared on the 2d day after delivery (which took place at 3 o'clock in the morning of the 15th), and the secretion of it continued. The lochial discharge had not been diminished since the commencement of the pain, which was first felt in the afternoon of the 19th, and appeared to come on from agitation and alarm, occasioned by some words that had been spoken in her presence, relative to a subject not necessary to be mentioned here. I understood that the labour had not been lingering. It was her first accouchement; but she had miscarried between the 2d and 3d month of pregnancy, four or five times. The patient is of a delicate and phlegmatic constitution. She had passed restless nights since her delivery, from the violence of the after pains, for which some medicines had been prescribed, supposed to be opiates. The bowels not having been opened since the 17th, and not considering the patient to be labouring under puerperal fever, which, as far as my experience goes, is preceded by a rigor, I only prescribed an opening mixture, composed of a decoction of senna and tamarinds, to which were added manna and sulphate of magnesia, one ounce and a half of which were directed to be taken every two hours. Thin gruel was ordered for diet, and the nurse enjoined to keep the patient as still as possible.

At 7 o'clock of the same evening, I visited the patient again. The pain had increased, and extended all over the abdomen to the back. No feces had been discharged. A common enema was ordered, and the mixture to be continued; only one dose had been taken at 6 o'clock. At 8 o'clock I repeated my visit. The injection had produced two very copious motions, dark and fetid, and there was some abatement of the pain. The pulse as full as before, and 140 in the minute. As I was going out of the house, I was called back and requested to see the patient again, as she had become suddenly worse. She was labouring under a severe rigor. I ordered warmth to be applied to the back and legs, and the mixture to be discontinued. I then left her for an hour, expecting that, on my return, I sould see a fully formed case of puerperal peritoneal inflammation.

At 9 o'clock, much heat had succeeded the rigor; the pain greatly increased; the whole of the abdomen very tender, and rather tumid; pulse 144, and very full. The lochial discharge was much diminished; the secretion of milk continued. Had the patient been bled in these circumstances, there would probably have been a total suppression of the one, and an interruption given to the secretion of the other. I have, besides, laid aside the use of the lancet for some years, in these cases. I therefore prescribed the following mixture.

℞ *Olei terebinthinæ rectificati ℥iss. Mellis despumati ʒii. Aquæ puræ ℥ii. ft. mistura, cujus capt. ægra partem 3tiam 2da quaque hora.*

I expected, as I had repeatedly experienced, that this medicine would produce a sufficiently antiphlogistic effect, and at the same time would impart such a stimulus to the constitution, as would serve to sustain the "*vis vitæ;*" for it should be observed that the patient, with all her inflammatory excitement, was complaining of extreme lowness, and felt as if she were sinking, which states conjoined, are characteristic of puerperal fever.

At 5 o'clock in the morning of the 21st, I found that a mistake had been made in exhibiting the medicine, one-third of the mixture was taken at 20 minutes past 10, and the whole of the remainder at 20 minutes past 12. I was rather pleased than otherwise at the mistake, well knowing that a speedier effect would follow. A *pot de chambre* was nearly filled with liquid feces, by the quick operation of the medicine. The patient looked pleasantly, saying, she felt quite relieved. The skin was bedewed with a copious perspiration, an effect which appears invariably to follow the exhibition of turpentine in cases of this description. Pulse 130, and soft; the secretion of milk and lochial discharge continue. She felt an appetite for food, but the same diet was ordered as before. Having to travel some distance from Nottingham, I did not see her again until 8 o'clock in the evening, but left directions to repeat the medicine every two hours, if the pain should return.

At 8 *p. m.*, found the patient complaining of chilliness and of more pain; pulse 140; some headach. This partial return of the complaint was ascribed to the presence of more persons in the room than was desirable. Having felt herself much better, the patient wished to see some of her friends. I ordered the breasts to be drawn, and greater stillness to be observed. The turpentine mixture was directed to be given as before, none of it having been taken during the day.

At 8 in the morning of the 22d, learnt that the whole of the mixture had been given before 1 o'clock; after which the patient slept for four hours, and awoke with an appetite for breakfast. Pulse 120; slight pain of the abdomen on pressure. Several copious stools had been passed; also much urine, of a dark colour.

At 12 at noon, found the patient going on well; pulse 116. Has taken no medicine since last night, but left directions to give it should the pain return. Secretion of milk going on. Lochial discharge nearly ceased.

At 4 *p. m.*, the patient was complaining of more pain, the seat of which was confined to the neighbourhood of the left groin. She had been troubled with hæmorrhoidal tumors for some years, and they were now very painful, probably irritated by the action of the turpentine on the rectum. Pulse 126; some pain in the head, and thirst; has had no stool since the last visit. One dose of the turpentine mixture was ordered to be given immediately, and another at 5 o'clock; and at 6 o'clock four leeches were ordered to be applied to the hæmorrhoidal tumours, and the groin to be fomented.

At 8 *p. m.*, the four leeches had relieved the painful state of the tumours. The bites had not bled more than one hour. The first dose of the mixture had procured two very full motions. The second dose was rejected by vomiting. Pulse 118; skin cool; some pain in the head, probably occasioned by the turpentine, which may perhaps produce a determination to that part. Milk porridge was ordered for diet, in addition to gruel. The breasts had been drawn, and the secretion of milk was copious. Ordered the nurse to foment the abdomen for an hour, and the medicine to be discontinued. Lochial discharge very slight, and colourless.

January 23d.—At 8 *a. m.*, found the patient complaining of uneasiness and pain of the bowels, occasioned, as she thought, by distension from wind. She had slept well; the pain at the groin was much abated; pulse 118. Two pills, consisting of 5 grains of *extr. colocynth.*, and of 5 grains of *pil. cum hydr.* were ordered to be taken immediately; and in two hours after, one ounce and a half of the cathartic mixture, which had been prescribed at my first visit. The dose was ordered to be repeated in two hours. There had been one copious evacuation from the bowels in the night.

At 5 *p. m.*, I learnt that after taking the pills at 8 in the morning, the patient fell asleep, and slept until one in the afternoon; consequently, the last dose of the cathartic decoction was not taken until 4 *p. m.* Considerable debility was complained of. The pain and uneasiness of the abdomen were entirely gone. One stool had been passed, and with it much flatus had been discharged. Appetite improving, and she takes with a relish arrow-root, tapioca, and milk porridge, alternately. Abundant secretion of milk; lochial discharge very trifling; pulse 114. Ordered the mixture to be discontinued.

At 9 in the evening, found the patient continuing to improve; pulse 108; much weakness is still felt; appetite good.

On my return from a ride into the country in the afternoon of the 24th, I learnt that a messenger had called, requesting my attendance on the lady as soon as possible. I had seen her in the morning, and found her going on well, although she had not passed a very good night.

At 5 *p. m.* I understood there had been considerable pain in the head, and some tendency to delirium. In order to discover if the turpentine had any share in producing this affection, I made particular inquiry as to any circumstances that might have arisen to create uneasiness of mind, and ascertained that she had been, on the preceding evening, indulging too freely in conversation on a subject that was distressing to her. Having therefore no doubt as to the cause of the disturbed state of the head, I prescribed ice and water to be applied to the forehead, and a dose of the cathartic decoction every two hours until my next visit. As the abdominal pain was entirely removed, some practitioners, on seeing approach the tendency to delirium, might suppose there was going to take place a metastasis to the brain; but being in the habit of seeing my puerperal patients recover under the use of the turpentine, I felt confident that these unpleasant symptoms would quickly be removed by the application of cold to the forehead.

At 9 *p. m.*, found the patient in a profuse perspiration. The cold applied to the head gave great relief, and she requested the constant application of the ice and water; pulse 108. Bowels had been well opened. Ordered that in future more attention should be observed to keep the patient quiet.

On the 25th, saw the patient twice. She made no complaint. The cathartic mixture was recommended to be taken occasionally. Has had but one stool. Prescribed four grains of calomel, and three of antimonial powder, to be taken at bedtime, and four scruples of *pulv. jalap. comp.* early in the morning.

In the evening of the 26th, the patient complained of pain of the left side of the abdomen, near the groin; pulse 102; tongue clean; skin cool; appetite good. The pill and powder had procured several motions, which looked healthy. She asked my consent to apply some leeches to the part affected with pain, to which request I did not object. It appeared that the patient had received an injury on the left groin and hip a week or two before her accouchement. She was thrown down when walking, with considerable violence, by a man who was running in the street, and the part was severely bruised. She did not put herself under medical care at that time, nor was she bled; but a female friend advised leeches, which, however, were not employed, and it was owing to their having been recommended which induced her to request their application now. Local bleeding was therefore particularly indicated, and four leeches were directed to be placed on the part affected with pain. One drachm of a saline powder, consisting of one part of cream of tartar, one of nitre, and of two parts of gum tragacanth and of sugar, was also prescribed every four hours.

On the 27th, the four leeches had been applied, and there was a considerable discharge of dark coloured blood. She slept well; appetite good; weak broth allowed; pulse 98; no stool since yesterday afternoon. Ordered to take half an ounce of castor oil, and to repeat the dose at three in the afternoon, if the bowels should not be opened before.

January 28th.—Only one dose of the oil was found necessary yesterday. She has taken about two drachms of it this morning, which have procured one natural stool. The lochial discharge, which had nearly ceased, *but never entirely so*, appeared in greater abundance in the course of the night; pulse 96; is free of pain; appetite good; prescribed a box of opening pills, consisting of *extr. colocynth.* ʒss. *Pulv. rhei.* ℈i. *Sapon. duri et pil. in hyd.* ā gr. v. into xii. pills, two of which were directed to be taken at bed-time occasionally. The saline powder and all other medicines to be discontinued.

Finding the patient, on the 29th, in a progressive state of amendment, and the pills agreeing well, I proposed discontinuing my attendance.

On the 31st I called on the patient, found her doing well, she having sat up the most part of yesterday.

In the above stated case of puerperal fever, it will be seen that the *oleum terebinthinæ* was given immediately on the invasion of the disease, and that it was the only antiphlogistic remedy that was relied on for the removal of it; the inflammatory symptoms, moreover, being subdued by that potent medicine in the course of a few hours.

Nottingham, Feb. 1, 1823.

In addition to the above, the publication of which has been unavoidably delayed, it may be thought necessary to state, that puerperal fever has prevailed in this town and neighbourhood during the winter that has just passed, epidemically; and there were several cases reported to have terminated fatally. At that period a considerable number of patients affected with that complaint, and also three under the influence of puerperal mania, fell under my care. Not any of those who were ill of the former disease, to whom I was called, were bled, either generally or topically, and every one of them recovered. Augmenting intestinal secretion appeared very speedily to put a stop to the complaint in every instance. From four to six leeches were applied to the temples of those affected with puerperal mania; also blisters to the scalp, and cold applications to the forehead. Sinapisms to the lower extremities, and cathartic clysters were also employed. The medicines given internally were calomel with antimony, camphor mixture with *aq. ammon. acetat.* &c. All those affected with this disease, whom I attended, also recovered.

As far as my experience goes, venesection in puerperal mania is quite inadmissible. I never knew a patient, whose case fell under my own observation, recover, who was subjected to that mode of treatment. The surgeons who were in attendance on two of the cases of puerperal mania, had very judiciously applied a few leeches to the temples, thinking thereby to remove a congested and overloaded state of the vessels of the brain that might occur from vascular action, violently exerted, forcing more than the usual quantity of blood to that organ.

It may be proper to state in conclusion, that many of those who have survived the late puerperal epidemic, and who were bled freely, are reported to enjoy but an indifferent state of health. A patient who resides in this town, and who, from her statement, appears to have lost an enormous quantity of blood in consequence of an inflammatory puerperal attack fourteen weeks ago, is now under my care. She ascribes her present indisposition to the repeated full bleedings with which she was then treated. She is now in a state of complete cachexia; the countenance is very pallid; much languor is felt; there is great emaciation; and the lower extremities are œdematous. I have advised a residence in the country, hoping that the constant exposure to pure air may improve the digestive powers of the stomach, and incite it to take more nourishment.

I have had an opportunity of seeing several patients who were treated with the turpentine, and they uniformly enjoy vigorous health. The lady, whose case is here given above, and in whom the disease may be said to have been sporadic, is again pregnant, after having given suck to her child till nine months of age. The cases of puerperal fever that have occurred to me since, have partaken of an epidemic character; but, to the best of my knowledge, neither the disease, nor the remedy which removed it, have left behind them any cause of complaint.

Nottingham, 5th May, 1824.

X.

On the Detection of Minute Quantities of Arsenic in Mixed Fluids.

By Robert Christison, M. D., F. R. S. E., Fellow of the Royal College of Physicians, Edinburgh, and Professor of Medical Jurisprudence in the University of Edinburgh.

It is well known, that the presence of various mineral vegetable and animal substances in fluids which contain arsenic, alters considerably the action of its liquid tests; and that their effects are occasionally so much modified as to supply no proof whatever of the existence of the poison. Toxicologists have attempted to get rid of these inconveniencies by making allowance for the modifications alluded to, or by removing the causes in which they originate; and some of the processes devised for these ends (if we may judge from their having obtained a place in every recent work on Medical Jurisprudence) are now held to form a necessary part of the medico-legal investigation of cases of poisoning by arsenic.

Having had occasion to repeat and vary the experiments, on which the proposed processes are founded, and having obtained results by no means conformable with those stated by authors, I was led to undertake a general inquiry into the whole subject of the detection of arsenic in mixed fluids. And I am not without hopes, that a summary of the chief facts which have occurred to me will be acceptable to medical jurists, in so far as they may correct some errors and oversights in authors, which have not hitherto been recognised, estimate fairly the value of the analytical processes recommended by different chemists, and introduce more precision and greater simplicity into the investigation of the more complicated cases of this kind of poisoning.

In the remarks I have to offer, I shall confine myself chiefly to the effect of vegetable and animal substances on the action of the tests. The modifications produced by such mineral substances as may exist in conjunction with arsenic, considered as an object of medico-legal inquiry, have already been fully ascertained by the labours of several distinguished experimenters, and ought to occasion no difficulty in the detection of the poison even to a person of but moderate practice in chemistry. But it is not so with matters belonging to the two organic kingdoms. The researches of Rose of Berlin, of Orfila at Paris, and of some other experimenters of less note, have shown, that the precipitates caused in arsenical solutions may be kept dissolved, or may be materially disguised by the coexistence of animal or vegetable principles. And the difficulties thus created are even more serious than the researches of these authors lead us to imagine. For their experiments have been confined in a great measure to simple fluids and infusions; whereas it will be shown in the sequel, that the more compound mixtures of them in daily use, as articles of food or drink, give rise to far greater embarrassment; and, indeed, affect the precipitates so much as to render most of the liquid tests absolutely useless. The inutility of the tests will be also farther shown by a fact, to which former experimenters have paid but little attention,—namely, that the very same precipitates are often produced in these composite fluids, whether arsenic be present or not.

It has been thought by some, that the inconveniencies now mentioned may be removed by destroying the colour of the liquid in which the poison is dissolved; and in this expectation several decolorizing processes have been proposed by chemists, and adopted in all works on Medical Jurisprudence. These processes I have investigated with some care, and if I mistake not, I shall be able to show, not only that such means are inadequate to accomplish the end in view, namely the restoration of the true colour of the precipitates; but likewise, that some of the tests will notwithstanding indicate arsenic where none exists, while others will fail to detect it where it does exist, nay, that a person who follows faithfully one of the processes recommended, may in some circumstances fail altogether to discover the poison where it is nevertheless present in considerable quantity. Other more complicated processes have likewise been suggested, with the view of transforming the oxide of arsenic into a different compound of the metal, and destroying at the same time the whole vegetable and animal matters originally present. These processes too have been a good deal relied on, and perhaps fully more than their accuracy and delicacy will justify.

In the following inquiry I shall examine, *first*, the changes in the action of the arsenical tests produced by the presence of various compound nutritive fluids; *secondly*, the value of the plans proposed for decolorizing such fluids, with the view of restoring the genuine action of the tests; and, *thirdly*, the utility of certain complicated processes for developing the poison, when it is so intimately united with organic substances, as not to be discoverable by the common re-agents.

In my experiments, I have confined my attention to mixtures containing the arsenic in small proportion, namely from two grains to a quarter of a grain in the ounce, or in round numbers, from a 250th to a 2000th part of the mass. These extremes will include by far the greater number of arsenical fluids which occur in the practice of Medical Jurisprudence. Besides, in solutions more concentrated than the strongest of those I have chosen, the action of the tests approaches pretty nearly that observed in pure arsenical solutions; and in the least concentrated, the indications (except by a single test only) are so obscure, that it was quite needless to examine those in a state of still greater dilution.

Section First.

When the oxide of arsenic is dissolved in mixed animal or vegetable fluids, its liquid tests become inaccurate on four accounts. Sometimes the colour of the precipitate, although in reality not altered, appears to be changed, because it is viewed through a medium of a different colour; and this fact is essentially important, because the operation of the test may not only be disguised, but also even imitated where no arsenic exists. Thus the blue precipitate, thrown down by the fixed alkalis in sulphate of copper, may resemble the arsenite of copper, if it floats in a yellow-coloured fluid. On this subject, some good observations are to be found in the treatise of Paris and Fonblanque on Medical Jurisprudence. (II. 247.) These gentlemen however have dwelt too strongly on this source of fallacy; for much more frequently the precipitate thrown down from an arsenical solution is really changed in colour, because animal or vegetable matter has been thrown down along with it. There is not one composite liquid, of all those I have examined, in which lime-water causes a white precipitate with arsenic; it has always more or less of a yellow or brown tint. The precipitate produced by the ammoniacal nitrate of silver has also generally a brown or black colour, which, in the stronger arsenical solutions, succeeds in a few seconds the proper yellow tint of the arsenite of silver, and in the weaker is struck immediately. Nay, even the sulphuretted hydrogen, which is less affected than the rest, instead of a bright sulphur-yellow precipitate, causes one with a shade of white, green, or red; at the same time it often decolorizes the fluid; and the precipitate, when dried and reduced in a tube, always exhales the fetid odour of burning animal or vegetable matter. Thirdly, the tests may be inaccurate in their indications, because the precipitate is kept suspended. Lime water is much exposed to this inconvenience; for the arsenite of lime is lighter than the other arsenites, and consequently, in fluids containing sugar, mucilage, albumen, caseïn, and the like, it is kept long in a state of suspension. Lastly, the tests may fail altogether, because the precipitate they cause with arsenic is soluble in the vegetable or animal principles of the fluid, and therefore does not separate. Lime water and the nitrate of silver are partially liable to this inconvenience; and the ammoniacal sulphate of copper for the same reason scarcely ever affords any indication whatever. This is owing, as Rose of Berlin first proved, to the arsenite of copper being very soluble in tannin.

These statements are supported and illustrated by the following experiments. The fluids employed were strong Broth, made with beef, barley, greens, and salt, Tea with sugar and cream, Coffee made in like manner, Porter, Port wine and Milk. The only tests I have thought it necessary to use are those in commonest use—the Lime water, the Ammoniacal Sulphate of Copper, the Ammoniacal Nitrate of Silver, and Sulphuretted Hydrogen.

Lime water.—In filtered Broth (which is hazy and of a very pale straw-yellow colour), Lime water darkens the colour, and causes a slight increase of the haziness. If one fourth or half a grain of arsenic exists in each ounce of the broth, the effect is not different; if a whole grain is present, the cloudiness is considerably greater, but the precipitate remains in suspension. In Coffee, (which after filtration is faintly translucent, whitish and opaline), lime-water causes transparency, but no other change. If the coffee contain from a 2000th to a 500th part of its weight of arsenic, scarcely any difference is observable in the effect of the test; a very faint haze merely being produced in the strongest solution: but if the proportion of arsenic be a 250th part, a pretty considerable, yellowish-brown flocculent precipitate is formed. In strong Tea, made with abundance of sugar and cream (which also is translucent, whitish and opaline after filtration), lime-water causes transparency, and deepens the colour, but has no other effect. If arsenic is present in the proportion of half a grain to the ounce, a haziness ensues, and in some hours a scanty yellowish-brown cloud falls to the bottom. This appearance is of course more distinct, when the proportion of arsenic is greater. In Porter, heated to expel the carbonic acid, lime water causes a copious flocculent precipitate, of a pale, dirty yellowish-brown colour: if it contains two grains of arsenic in the ounce, the effect is not different, except that the precipitate is somewhat more abundant. In Port wine it causes a copious, smoke-gray, flocculent precipitate: if the wine contains two grains of the oxide in every ounce, the precipitate is nearly the same in quantity, but dirty reddish-gray in colour. In Milk it has no other effect than we should anticipate from the necessary dilution;—if arsenic be present to the extent of two grains in the ounce, the effect is not different.

Hence it is clear, that, in animal or vegetable fluids containing a moderate proportion of arsenic, Lime water is totally inapplicable as a test, because it sometimes produces no precipitation at all; at other times causes the same precipitate, as in an arsenical solution, although no arsenic be present; and never throws down the arsenite of lime of its characteristic white colour.

Ammoniacal Sulphate of Copper.—The ammoniacal sulphate of copper turns filtered Broth to grass-green, without causing any precipitation. If arsenic be present in the proportion of a 500th, 1000th, or 2000th part, it has no other effect immediately; but in six or eight hours a minute yellowish-green precipitate falls down. In Coffee the same test causes a yellowish-green colour, and no precipitate. If arsenic be present, the green tint of the colour predominates; but no precipitate is caused though two grains be dissolved in each ounce. In Tea it causes an intense opacity and a deep smoke-brown colour, but no proper precipitation; when arsenic exists in the proportion of a 250th or 1000th part, the colour partakes more or less of green, but no true precipitate is formed. In Porter it produces a copious deep leek-green precipitate, which becomes dingy on standing. If arsenic be added, even to the extent of a 250th part, the effect is precisely the same. In Port wine it causes a dark-brown opacity, from which a copious greenish-brown precipitate slowly separates. The presence of two grains of the arsenical oxide in each ounce causes no difference. In Milk it strikes a beautiful azure blue colour, but causes no precipitation: in the weaker solutions of arsenic in milk it has the same effect; but if the proportion be one grain to the ounce, the colour passes in a few hours to dirty celandine-green; and if the proportion is double, this tint is struck at once; yet in both cases without any obvious precipitation.

The ammoniacal sulphate of copper is therefore quite as inapplicable to these fluids, as lime water; for it sometimes causes no precipitate where arsenic abounds, and at other times, when no arsenic is present, occasions a precipitate very similar to that which it produces in an arsenical solution.

Ammoniacal Nitrate of Silver.—The ammoniacal nitrate of silver causes in filtered broth an abundant cream-coloured precipitate; which, on the addition of nitric acid, is changed to snow-white. The effect is not in the slightest degree different, if arsenic be present to the extent of a grain in the ounce. In Coffee it occasions a pale chestnut-brown precipitate, passing in two or three seconds to deep grayish-black. If arsenic be dissolved to the extent of two grains in the ounce the immediate effect is the same; but the change of colour takes place more slowly; and the rapidity of the change is inversely as the quantity of arsenic. In Tea it occasions a reddish-brown opacity, so intense that it is not easy to say whether a precipitate is formed or not; but none falls down. If arsenic be present, an abundant precipitate forms, which in the weaker solutions is chestnut-brown, passing rapidly to deep grayish-black, and in the strongest has a brownish-yellow tint, and undergoes the same change much more slowly. In Porter it produces a copious dirty grayish-yellow precipitate, passing in two or three minutes to deep grayish-black. If arsenic be present in the proportion of two grains to the ounce, the precipitate is greenish-yellow, but as permanent as that thrown down in a pure arsenical solution. In Port wine this test causes a copious precipitate in intermixed white and brown streaks, becoming immediately brownish-black, and after some hours chestnut-brown. When two grains of arsenic were present in every ounce, the precipitate was at first dirty-white, but passed very slowly, through ash-gray to dark sienna-brown. In Milk it causes an imperfect coagulation, and renders the colour more purely white: if arsenic exists in the milk even in the proportion of a 2000th part, the tint verges on yellow, being in the weakest mixtures cream-white, and in the strongest sulphur-yellow; but the precipitate remains always suspended.

Thus, then, on arsenic, dissolved in coffee, porter, broth, or port wine, this test has scarcely any other effect than it has on the same fluids, when no arsenic is dissolved in them: in tea the precipitate is materially disguised in colour; and in milk it is kept in suspension. Consequently, notwithstanding the restless and unwearied anxiety shown of late by some to uphold its excellence, the indications it gives are no more to be trusted in such fluids, than those of the two former re-agents.

Sulphuretted Hydrogen.—A stream[*] of sulphuretted hydrogen gas passed through Broth, Coffee, Tea, Port wine, or Milk, * has no effect, whether acetic acid be previously added or not; † neither has it any immediate effect on Porter; but a very scanty dirty reddish-gray precipitate is afterwards deposited on the application of heat. In Broth, containing only a 2000th part of arsenic, the gas first strikes a bright lemon-yellow colour, and, in less than fifteen minutes, a precipitate falls down—copious, flocculent, and of the same bright lemon-yellow colour. If the quantity of arsenic be greater, the precipitate has a sulphur-yellow tint, and is thrown down at once. In Coffee the same test produces, with a 2000th part of arsenic, an abundant grayish-yellow precipitate, becoming bright lemon-yellow on the application of heat; and the precipitate is at once struck of this colour, or sulphur-yellow, if the proportion of arsenic is greater. In Tea it likewise causes a copious lemon-yellow precipitate, with a 2000th part of arsenic; but the tint is not characteristically bright till the fluid is heated; with a 1000th part the tint is at once a full lemon-yellow; with a 500th part sulphur-yellow. In porter, which contains a 2000th part of arsenic, it occasions a grayish-yellow milkiness, which, on the application of heat, becomes an abundant dingy chestnut-brown, and flocculent precipitate. If the quantity of arsenic is greater, a dingy lemon-yellow precipitate is thrown down at once; and its colour is somewhat brightened by the application of heat. In port wine, containing a 2000th part of arsenic, the precipitate is reddish-brown, with a 250th part orange-red. In milk containing a 2000th part of the oxide, and deprived of its cheesy matter by acetic acid, the gas produces a dingy lemon-yellow cloudiness, which, on the application of gentle heat, becomes a flocculent precipitate of the same colour; if the proportion be greater, the colour is sulphur-yellow; if it be a 250th of the mass, the precipitate is thrown down at once without the aid of heat. ‡

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[*] Most authors recommend, that the sulphuretted hydrogen, when used as a test for arsenic, be previously dissolved in water; and this mode of applying it seems at present to be generally preferred. A stream of the gas acts with equal delicacy, possesses the advantage of not diluting the solution, and is much more convenient; for, as its solution in water cannot be kept for occasional use, it must be prepared afresh each time it is required.

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* On heating milk deprived of its coagulable matter by acetic acid, and then impregnated with sulphuretted hydrogen, a dirty-grayish flocculent matter is separated. With this phenomenon, however, the test has no concern, as it takes place equally though no gas be previously transmitted. It takes place, in fact, because the acetic acid cannot coagulate the whole of the caseïn in the cold.

† Before applying this test to arsenical solutions, it is necessary to add acetic acid to them, provided an alkali exist along with the arsenic, otherwise no precipitate takes place. Phosphate of soda has the same effect as an alkali, probably on account of the excess of soda it contains; but in this case, too, the action of the test is perfected by the previous addition of acetic acid to the suspected fluid. Hence the acid is often requisite in animal and vegetable solutions, as they frequently contain an excess of alkali.

‡ In these experiments with milk, care was taken not to raise the heat much above 150°, for then the portion of caseïn, which the acetic acid could not throw down in the cold, was deposited in such quantity as to obscure greatly the colour

It is hence apparent, that of all the tests the sulphuretted hydrogen is by far the most accurate, or rather the only one at all useful in composite animal or vegetable fluids; that its action on arsenic is neither imitated nor prevented, and sometimes not materially modified, by any organic principle existing in these fluids: and that it is sufficiently delicate for all medico-legal purposes, since it acts (and in general characteristically), on arsenic, when dissolved in 2000 parts of the most complicated and deeply-coloured fluids. I have not thought it requisite to determine the limit of its delicacy in such fluids, because I conceive the medical jurist will have scarcely ever to search for the poison, when it exists in a state of greater dilution. I may mention, however, that I have often found the indications of the test distinct, and always procured some precipitate when the arsenical oxide was dissolved in 8000 parts of the most complex fluids. I shall afterwards show, in the Third Section, how the sulphuretted hydrogen may be employed, so as to yield, without the assistance of any other, the most decisive and unequivocal proofs of the presence of arsenic in almost every possible case.

The foregoing statements, however, establish, beyond all doubt, the inadequacy of the fluid tests for arsenic, when it exists in moderately diluted solutions, along with vegetable or animal principles. For of the four most accurate tests, only one yields indications which can be trusted to at all; and even that test will sometimes fail to act characteristically.

Section Second.

Several modes have been proposed for destroying the colour of these animal and vegetable infusions, under the expectation that the precipitates produced by the tests would then appear of their true and peculiar tints. Two processes have been recommended for this purpose. The one was proposed in 1821, by Professor Orfila of Paris, in his Leçons de Médecine-Légale. The other was published last January in the Annals of Philosophy, by Mr Rich. Phillips, editor of that journal. Orfila proposes to destroy the colour by chlorine; after which, he alleges the re-agents act on the filtered fluid as on a pure arsenical solution (II. 93. & 416). Mr Phillips, on the other hand,

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of the precipitate. I may add, that milk not deprived of its cheesy matter, although it contains a slight excess of acid, possesses some solvent power over the sulphuret of arsenic; a stream of the gas causes scarcely any perceptible yellowness, unless the quantity of arsenic exceeds a grain to the ounce; yet, when the acetic acid is dropped into such a mixture impregnated with the gas, it becomes at once bright yellow.

proposes to boil the suspected fluid with animal charcoal; and says, he has found, that port-wine, gravy-soup, infusion of onions, or the *liquor arsenicalis*, may be rendered in that way sufficiently colourless for the application of the most delicate tests (vii. 31. N. S.) These processes do not seem to have been yet subjected to experiment by other chemists.* I can conceive no other reason, at least, why the former in particular has been so long under the public eye without notice or criticism. For no one can have made fair trial of them, without being convinced that their application is confined by such narrow limits, and that, within those limits, they are liable to so many fallacies, as must render them almost entirely useless in medico-legal researches.

If I may be permitted to judge from a great number of experiments made repeatedly and with due care on several kinds of coloured fluids,—which were chosen at random from among the common articles of food and drink,—the process of Orfila is objectionable on the three following grounds:

1*st*, The colour is very seldom destroyed so entirely but that the precipitates caused by some of the tests still deviate to a certain degree from their characteristic tints; and although the colour of the fluid be even destroyed entirely, it often re-appears in the precipitates.

2*ndly*, Although the chlorine destroys the colour, it does not also take from the fluid its power of preventing the action of some of the tests—in other words, its power of retaining the arsenical precipitates in solution.

3*dly*, In fluids decolorized by chlorine, and containing no arsenic, some of the tests produce precipitates, not only precisely the same with those which they cause in a decolorized arsenical solution, but likewise very similar in appearance to those caused in a pure aqueous solution of arsenic.†

The subjoined experiments will show that lime-water is very liable to the difficulties mentioned in the first of these objections, the ammoniacal nitrate of silver and ammoniacal sulphate of copper to that related under the second, and lime-water and the ammoniacal sulphate of copper to that most important of all laid down under the third. In short, the only test whose efficacy can be relied on in decolorized arsenical fluids, is the sulphuretted hydrogen. Now, we have already seen, that the sulphuretted hydrogen acts with great nicety and exactness on arsenical solutions, notwithstanding the conjunct presence of the most complicated and deeply coloured organic principles. Hence the process of decolorization is unnecessary, so far as regards that test; nay, it is rather injurious than useful; for, unless the arsenic exists in large proportion, the bright golden colour of the sulphuret is sometimes deadened to lemon-yellow, or even yellowish-white.

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* That of Orfila, indeed, has been noticed in the late elaborate work of Dr Paris and Mr Fonblanque; who admit that it may aid the accomplished chemist; yet declare, that, in the hands of an unpractised person, it is subject to fatal fallacies. But they assign no reason for this opinion, and do not appear to have personally tried the process. (See *Med. Jur.* ii. 253.)

† Orfila has paid no attention to the effects of the tests on decolorized fluids containing no arsenic.

The fluids chosen for the experiments about to be related, were Port wine, Porter, and Tea made in the usual way with cream and sugar. They were decolorized sometimes by the liquid chlorine, as advised by Orfila, sometimes by a stream of the gas itself; but ere long I always preferred the latter mode, as being less troublesome, more complete in its effect, and possessing the advantage of not diluting the fluid. It always separated a curdy precipitate, which of course was removed by filtration before the tests were applied. After filtration, the port wine had commonly a very faint wine-yellow tint; the porter, though it had entirely lost its brown colour, had a milky opalescence; but the tea was, in general, decolorized altogether.

Lime-water.—In decolorized Port wine, lime-water produced a pretty considerable flocculent precipitate of a pale yellowish-red colour. * If arsenic was present to the extent of two grains in the ounce, the precipitate was more abundant, but of the same colour, or sometimes flesh-red. In decolorized Porter, it first destroyed the milkiness produced by the action of the chlorine, and then caused a pale yellowish-gray cloud, which gradually formed into a flocculent precipitate of the same hue. If arsenic was present, even to the extent of a 250th part, the effect was precisely the same, except that the quantity of the precipitate was considerably greater, probably twice as great. On decolorized Tea, lime-water had no effect whatever. When it contained from a thousandth to a 250th of arsenic, a pale yellowish-brown cloud was formed; and in the course of some hours, a scanty flocculent precipitate of the same colour gradually fell down.

In none of these mixtures, then, after they are deprived of their colour, does that re-agent produce its characteristic effects; and in two of them, its effects, though differing in degree, are the same in kind, whether arsenic be present or not.

Ammoniacal sulphate of copper.—The ammoniacal sulphate of copper, added to decolorized Port wine, first causes an apple-green precipitate, which is speedily re-dissolved; but, on the addition of a larger quantity of the test, a copious bluish-green precipitate is thrown down, which is permanent; and, in the course of some hours, passes to celandine-green, and thence in a day to a dingy tint, intermediate between celandine and apple-green. * The effects are precisely the same in every respect when arsenic is present, even to the amount of a 250th part. In fact, the arsenite of copper is not thrown down at all in the decolorized arsenical solution; on the contrary, decolorized port wine dissolves the arsenite of copper thrown down from a pure solution of arsenic. This is probably owing to the tartaric acid contained in the wine; for the arsenite of copper is very soluble in that acid. † On decolorized Porter, with or without the oxide of arsenic, the effects of the ammoniacal sulphate of copper are precisely the same as in the case of port wine;—with the single exception, that after the precipitate ceases to be re-dissolved by the fluid, it is thrown down celandine-green at once. The very same remarks apply to decolorized Tea, with or without arsenic. The solvent in porter is probably, as in wine, the tartaric acid; in tea it is tannin, according to Rose.

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* Impure tartrate of lime.

These experiments require no commentary to enable the reader to understand the danger, that, in such circumstances, might accrue from the use of this test, sanctioned as it is by the authority of Orfila. It is, in fact, altogether deceptive. Not only does it not throw down arsenic, which does exist in the mixture; but also, when no arsenic is present, it causes a precipitate very similar to the arsenite of copper.

Ammoniacal Nitrate of Silver.—The ammoniacal nitrate of silver causes with decolorized Port wine an abundant white precipitate, having a very faint tint of red, and permanent in the dark. When arsenic exists to the amount of two grains in the ounce the effect is precisely the same, there is not the slightest perceptible tint of yellow. ‡ This is not owing to the great quantity of chloride of silver obscuring the colour of the arsenite; nor does it arise from the disengaged nitric acid of the test retaining the arsenite in solution; for if a very small proportion of arsenic be dissolved expressly in a solution containing free hydrochloric acid, the precipitate with the ammoniacal nitrate of silver has a deep yellow tint. It is owing to the solvent power of the wine itself; for decolorized wine, if the chlorine and hydrochloric acid be previously removed by nitrate of silver, readily dissolves the arsenite of that metal. The dissolving principle of the wine, as in the case of the last re-agent, is probably the tartaric acid; at least a small quantity of tartaric acid rapidly dissolves the arsenite of silver. * In decolorized Porter, the effects of the ammoniacal nitrate of silver are almost exactly the same; the colour of the precipitate is cream-white, or grayish-white, without any reference to the arsenical impregnation of the fluid. The same remarks apply to decolorized Tea; the precipitate, however, being in all circumstances snow-white. I have not had leisure to inquire what is the solvent principle in the last case. In porter, as in wine, it is probably the tartaric acid.

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* I have not had leisure to inquire what principles in the wine occasion this precipitate.

† It may be also kept in solution by the chlorine changed to hydrochloric acid, provided the test contain but a small proportion of ammonia insufficient to neutralize that acid. The arsenite of copper is also exceedingly soluble in nitric acid and acetic acid: oxalic acid in minute quantity also dissolves it; but, in a few seconds, a pale azure-blue precipitate falls down, the oxalate of copper. Hence it is easy to see, that when the quantity of arsenic is inconsiderable, it will seldom be thrown down from vegetable infusions by the ammoniacal sulphate of copper.

‡ Of course, care was taken to continue the addition of the test after all the chlorine had been thrown down and removed by filtration.

It is evident, therefore, that in regard to this test also, the process of decolorization by chlorine is quite useless; because it does not enable the test to act upon the arsenic at all.

4. *Sulphuretted Hydrogen.*—On decolorized Port wine sulphuretted hydrogen has no immediate effect; but in a few minutes it causes a yellowish white milkiness, which, on the application of heat, gives place to a considerable reddish-gray, dingy precipitate. † The precipitate is sensibly less, if the fluid, before the transmission of the sulphuretted hydrogen, be heated to the boiling temperature, so as to drive off the chlorine: yet even then the quantity is not inconsiderable. When arsenic is present, even in the proportion of a 250th part, the sulphuretted hydrogen has at first no effect; but in a few minutes a yellowish-white opacity takes place, and the application of heat then produces a copious yellowish-white precipitate. The effect of this test on decolorized Porter is nearly the same as on decolorized wine; except, that, when no arsenic is present, no change takes place before heat is applied, and when arsenic is present, the precipitate is lemon-yellow, instead of yellowish-white. On decolorized Tea, with or without arenic, its effect is precisely the same as on porter. ‡

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* The arsenite of silver is also soluble in citric and acetic acids, but not so readily: oxalic acid likewise dissolves it, but, if an excess be not used, a white precipitate is soon substituted, the oxalate of silver.

† This precipitate is sulphur, tinged by the remaining colouring matter of the wine, and derived from the decomposition of the sulphuretted-hydrogen by the chlorine.

‡ In all these experiments, except in the case of wine, the action of the gas on the arsenic was greatly facilitated by the previous addition of a little acetic acid. The reason of this is not very obvious.

From these experiments it would appear, that Orfila's process does not render the action of this test more true; for the colour of the sulphuret of arsenic must be modified in the weaker solutions by the precipitate caused by the test with the chlorine alone; and in the stronger solutions no decolorizing process is required, since the sulphuretted hydrogen acts characteristically without its intervention.

If the foregoing observations are correct,—and neither time nor labour has been spared in varying the experiments, on which they are founded,—it follows, that, in the circumstances in which a decolorizing process is requisite, namely, in solutions not very concentrated, the method of Orfila is generally useless, often detrimental, nay, sometimes even dangerous. The reader may therefore be naturally disposed to inquire, why it has been recommended by so reputable an author, and so practised a chemist as the Parisian Professor. One cause of the errors into which he has fallen has evidently been an inattention to the effects of the tests on decolorized fluids which contain no arsenic. Had he investigated that point, he could not have failed to observe, that the lime water and ammoniacal sulphate of copper act on such fluids when unsophisticated, nearly or entirely in the same way as when arsenic is present in them. As to the silver test, which we have seen to be so useless in decolorized fluids,—if we may judge from the imperfect account of it given in his works, it has never been much the object of his regard. I may add further, that, if, according to the practice of most lecturers on chemistry, he employed very strong solutions of arsenic, he would undoubtedly find the tests very characteristic in their action. But such solutions can very rarely be the subject of medico-legal investigation; and, besides, most of the tests would act as characteristically on them, without the previous destruction of their colour.

The next mode in which it has been proposed to decolorize mixed arsenical solutions, with the view of restoring the true action of the tests, is by boiling them with animal charcoal. The method recommended by Mr Phillips, the proposer of the process, is thus stated in the number of the Annals of Philosophy for last January.

"On considering this part of the subject, it appeared to me that animal charcoal (ivory-black) might be advantageously employed for the purpose of destroying the colouring matter. I therefore mixed some of it with a coloured solution of the arsenious acid, namely, the *liquor arsenicalis* of the London Pharmacopœia. I found that the colouring matter was so completely destroyed in a few minutes, that the test of nitrate of silver, or any other, might be readily applied. I repeated this experiment with port wine, gravy soup, and a strong infusion of onions, and succeeded in these cases in procuring a solution sufficiently colourless for the application of the most delicate tests." (p. 31.)

Since animal charcoal possesses the property of removing mineral, as well as vegetable and animal matters, from the fluids in which they are dissolved, it might be reasonably questioned, whether by removing the colour of such fluids the arsenic will not be removed also. And the doubt here expressed carries some weight with it, when we consider that not long ago Mr A. T. Thomson proposed to detect arsenic in solution, by boiling the fluid with charcoal, and then collecting and drying the charcoal, and subjecting it to the heat necessary to sublime the metal. Accordingly I have found, that from all the solutions of arsenic in the coloured fluids formerly mentioned, the poison is nearly or entirely removed, before the colour is destroyed by Mr Phillips's process. If coffee, which contains two grains of the oxide in each ounce, be digested with successive small portions of animal charcoal till its colour is nearly removed, the sulphuretted-hydrogen is the only test which has any decided effect on the resulting fluid; it causes a very scanty lemon-yellow precipitate. Lime water has no effect, the copper test produces an exceedingly scanty azure-blue, and the silver test an abundant cream-white precipitate. Tea decolorized in the same manner gives no distinct indications with any of the tests, except sulphuretted hydrogen, if it contained but one grain to the ounce. If the quantity, however, had been twice as great, then the sulphureous precipitate is abundant, and the silver test gives a yellowish white one, which becomes white with nitric acid, and the remaining solution gives a pure gamboge yellow precipitate, after filtration and neutralization of the ammonia by nitric acid. Lime water, however, does not act distinctly; and the copper test merely gives the colourless liquor a bright grass-green tint, without causing any precipitation. Porter, even when it contains two grains per ounce, gives no precipitate with lime water after decolorization; the copper test causes a scanty azure precipitate, passing in half an hour to celandine-green; the silver test a pale cream-white one; but the sulphuretted hydrogen a pretty considerable sulphur-yellow powder.

These experiments render it clear, that in fluids so deeply coloured as the foregoing, the destruction of their colour will seldom restore the genuine action of the tests, provided the proportion of the poison do not exceed a 250th part. I have already said, that in medico-legal analyses solutions of greater strength will seldom be met with.

It was hardly necessary to search for the cause of the failure of Mr Phillips's process. But that no doubt might be left on the subject, I submitted to Mr Thomson's test the charcoal on which the coffee and tea had been digested: and though the quantity of charcoal was considerable, I procured from it, with proper precautions, a large quantity of metallic arsenic.

Another objection also lies against this process. Although it removes one cause of the inaccuracy of the tests, namely the colour of the fluids in which the arsenic is dissolved, it does not always remove another source of embarrassment, namely the power which some of these fluids have of retaining the precipitates in solution. This is well exemplified in the case of tea: for in a decolorized solution of arsenic in tea, of such strength as to yield decided indications with the silver and sulphureous tests, the ammoniacal sulphate of copper causes no precipitate, but merely strikes a green colour. Other instances of the same nature might, I am sure, be discovered; but I have not had leisure to pursue the subject farther.

It appears, therefore, that in mixed arsenical fluids, of the strength commonly to be met with in medico-legal researches, no advantage, but rather positive harm, will result from the employment of the processes at present in use for destroying the colour of the menstruum.

Section Third.

It now remains for me to examine in the third place the processes which have been suggested for the detection of arsenic, when it is so minutely united with animal or vegetable matters, as to resist the action of ordinary reagents. These processes may be arranged under two heads. One consists in the simultaneous deflagration of the organized matter, and conversion of the arsenic into arsenic acid. The other includes several processes for effecting the solution of the whole compound matter, and then throwing down the arsenic in the form of some arsenite or other, with the view of subjecting it afterwards to the test of reduction.

The first process was originally proposed by a German chemist of the name of Rapp, and has been subsequently modified and recommended by Professor Orfila. It consists in evaporating the mixture to dryness; and projecting the dried mass in small successive fragments into melted nitre, contained in a narrow-necked mattrass. In this way, while the organic matter undergoes deflagration, the arsenic is said to be more highly oxidated, and to form, with part of the free potass of the decomposed nitre, an arseniate of potass. After this stage of the process is completed, it only remains to dissolve the mass in water, and neutralize the rest of the free potass with nitric acid; and then the arsenic acid may be detected by its liquid tests, which are sufficiently characteristic. The applicability of this process evidently depends on the supposition, that the carbonaceous matter of the mixture will take its oxygen rather from the nitre than from the oxide of arsenic. One would be apt to imagine, however, that the carbonaceous matter, when heated to redness on the surface of the fused nitre, might take its oxygen also from the oxide of arsenic, so that the latter would be immediately sublimed in the form of metallic arsenic. From several very careful experiments, conducted with all the precautions mentioned by Orfila, I am disposed to believe that this effect is really often produced, if the proportion of carbonaceous matter be considerable, or at all events, that, whatever may become of the arsenic, it will not be found in the form of an arseniate in the fused mass, which remains in the bottom of the vessel. Thus I twice subjected to the process the dry residue of an ounce of tea, * in which a grain of arsenic had been dissolved; and I could not detect a particle of arsenic acid in the remaining mass. In a third trial I collected all the matter which had been raised and condensed on the upper portion of the mattrass, and which contained but a small proportion of charcoal; and I found, that when this was subjected anew to deflagration, arsenic acid might be detected in it in very minute proportion. I am not yet satisfied of the precise chemical changes which take place during the deflagration of mixtures of oxide of arsenic and dry vegetable and animal matter. But from the trials already made I conceive it may be fairly inferred, that when the quantity of the poison is small, and its proportion to the mixed matters likewise small, (in which circumstances alone, as will soon appear, the destruction of the organic matter is required), the mode of procedure advised by Orfila is quite useless, or at all events demands such nice manipulation as renders it altogether ineligible in medico-legal researches.

The next process I shall mention is that which was proposed by Rose of Berlin to be applied to the tissues of the stomach, and which, with certain modifications, may be also applied to every case in which the ordinary reagents fail.

Under the idea, that arsenic, when introduced into the stomach, will combine so intimately with its tissues as not to be separable by simply boiling it with water, he proposes to cut the whole organ into small fragments, then to boil them in a solution of caustic potass till they are almost or altogether dis-

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* Evaporated in the vapour-bath.

solved, next to neutralize the potass by nitric acid, next to throw down the arsenic with lime water, and finally to subject the precipitate to the test of reduction. The various niceties required in the management of this process I need not enumerate: they are to be found in every text book on Medical Jurisprudence.

The process of Rose I imagine will be found much superior to that of Rapp and Orfila, when the substance with which the poison has entered into combination, is (like the tissues of the stomach and most of the soft animal solids) soluble in caustic potass.

But is either of the processes ever necessary? Can the oxide of arsenic unite under any circumstances with vegetable or animal matter, so as to be inseparable from them by simple digestion in water? For, if no such union is possible, the ordinary mode of analysis will serve in all the cases for which these more complicated methods are intended. The question now stated has not hitherto been carefully considered; and indeed does not appear even to have occurred to some of those who have been most active in devising the processes alluded to. The only well authenticated evidence of a chemical action existing between organized matters and the oxide of arsenic is, that it retards or prevents their putrefaction. That such is the fact has been proved by several French and German experimenters; and I may add, that some observations I have lately made coincide with the result of their researches. But although this property should be assumed as proof of the existence of some chemical action and union between arsenic and organized matter, I am not acquainted with any fact or even analogy, which will lead to the inference, that a union of this kind, effected without change in the composition or structure of the combining matters, may not be overturned by the simple dissolving power of water or other ordinary solvents. I am not aware, that direct experiments have yet been made to show, that, in any of the soft animal solids, which have been preserved in arsenic and impregnated with it, the poison can be detected by the method of Rose, or that of Rapp, or in any other way whatsoever, after these impregnated solids have been thoroughly digested in boiling water. In the original memoir of Rose, indeed, he states that by means of his process he detected minute portions of arsenic in the stomachs of animals killed by it.* But these experiments are not at all conclusive. For he subjected to the action of potass not only the stomach itself, but likewise its whole contents; and without digesting them previously in water. This fact seems to have been overlooked by those who have quoted his treatise and adopted his proposed method. Others, I believe, have repeated his experiments with success, but under what precise circumstances I have not been able to learn.

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* Gehlen's Journal, II. 670. It appears that he sometimes succeeded in detecting so small a quantity as an eighth part of a grain.

I therefore thought it right, in the absence of satisfactory information, to make some inquiry into the nature of the supposed union of arsenic with animal matters,—so far at least as it is connected with medico-legal analyses. Having kept several portions of an ox's stomach in a saturated solution of arsenic for a fortnight, they were finely minced, and then boiled in successive portions of water, till the ammoniacal nitrate of silver had no effect on the washings. The residue was then treated with great care according to Rose's process; but I procured only a very minute flocculent precipitate, far too scanty to be collected. I next made another trial with three times the same quantity of an ox's stomach, which had been kept for a month in a saturated solution; but I procured no precipitate at all. Finally, I repeated the experiment with a considerable portion of a human stomach, which had been kept in a similar solution for three weeks; and the lime water merely produced a very slight haziness, but no precipitate. Of course in these experiments care was taken to add the lime water in great abundance, lest any deception might arise from the solubility of the arsenite of lime in an excess of arsenic.

We are entitled to infer from the foregoing results, that under any circumstances which can occur in the practice of Medical Jurisprudence, arsenic cannot be so united with animal matters as to be inseparable by simple digestion. Now, it will be shown presently, that when arsenic is once dissolved, whatever may be the vegetable or animal principles coexisting with it in the solution, one of the common liquid tests may be employed, so as to yield the most unequivocal proof of the existence of the poison, even when the quantity is inferior to that which can be detected by the method of Rose or Rapp. It is consequently fair to conclude, that both these methods are unnecessary; a result of no small importance, when it is considered how ill they are calculated to succeed in the hands of persons unaccustomed to chemical analysis.

The next process to be noticed is mentioned by Dr Paris in his work on Medical Jurisprudence. It is mentioned cursorily indeed; but he seems to place much reliance on it. I should observe, that it is applicable only to fluid mixtures. Dr Paris proposes to add the ammoniacal nitrate of silver to the fluid as long as any precipitate falls down, and to subject the dried precipitate to the process of reduction. This proposal may be objected to on several grounds; 1. In the first place we have seen that the arsenite of silver is soluble in various principles existing in animal and vegetable fluids; and consequently the arsenic may not be thrown down, though present in notable quantity; 2. As the test precipitates with almost all such fluids, whether arsenic be present in them or not, we shall be always put to the trouble of carrying on a tedious process to its very conclusion; because it is only at the very end, that we can obtain even a presumption of the presence or absence of the poison; 3. I have always found that after the fluids in question have been precipitated by nitrate of silver, the filtration of so small a quantity as two or three ounces requires a day and a half to be completed; and the solid matter when dry is so incorporated with the filter, that, if the quantity be small, little or none can be separated; 4. The quantity of animal and vegetable matter thrown down with the arsenite of silver is inconveniently great, and occasions much trouble during the subsequent process of reduction, by frothing up and filling the tube before the metal can be sublimed.

I shall now conclude with a description of what appears to me the most simple and unerring mode of detecting arsenic, when mixed with vegetable or animal solids and fluids, or, I may also add, when it is mingled with any *mineral* substance with which it cannot form, under the action of boiling water, an insoluble compound.

The agent I should propose to employ for separating it from them is the sulphuretted hydrogen gas. The facility which this test affords in the analysis of arsenical mixtures has been far too much neglected, especially in this country, where the attention of chemists and medical jurists has been rivetted on the two most inaccurate and most fallacious of the common reagents, namely, the ammoniacal sulphate of copper and the ammoniacal nitrate of silver. Neither of these tests is of any use in the greater number of vegetable and animal fluids, unless the poison be dissolved in them in such proportion as no one need ever expect to encounter in medico-legal investigations.

The suspected matter, if solid, † is to be divided into minute fragments, and boiled briskly in two or three successive portions of pure water. The fluid, whether originally such, or procured by digestion from the solid matter, is then to be subjected (in a deep narrow glass) for half an hour, or three quarters of an hour, to a brisk stream of sulphuretted-hydrogen gas. In many cases, however, it will be requisite to premise the two following preparatory steps, before transmitting the gas shrough the fluid;

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† I have not tried whether arsenic may be separated by digestion in water from all animal and vegetable solids with which it may be mixed; but judging from the experiments I have already made, I do not anticipate that greater difficulty will ever be encountered than when it has been intimately diffused through the tissues of the stomach.

and, as we can seldom know beforehand whether these steps are requisite or not, it may be proper to resort to them in every case. The first precaution is to add a little acetic acid to the fluid: By so doing the influence of any free alkali that may exist in it is counteracted; and several organic principles, which might impede the subsequent separation of the precipitate, are coagulated. The second precaution is to boil the fluid for a few minutes; by which means some matters are separated that the acetic acid could not throw down entirely; and any carbonic acid existing in it is driven off. The presence of carbonic acid in considerable quantity, by impeding the solution of the sulphuretted hydrogen, prevents its action on the arsenic, if the proportion of arsenic be small.* The fluid is then to be filtered. If the addition of acetic acid separated a large quantity of insoluble matter, it may be right to filter also before boiling. Whenever the quantity of insoluble matter separated by heat or the acetic acid is considerable, it must be removed from the filter after being well drained, and then squeezed in a linen cloth, and moistened with water and squeezed again, and the strained fluid added to the rest. When the stream has been continued a sufficient length of time, there is either a precipitate formed, or the fluid acquires a yellowish milkiness, which passes to a distinct precipitate as soon as the excess of sulphuretted hydrogen is driven off by heat. It is always right to boil before attempting to separate the matter thrown down, as the precipitate then becomes much more distinct and falls to the bottom more readily. The vessel is now to be left at rest in a cool place till the precipitate has completely subsided; as much of the supernatant liquid is to be poured off as is possible, and the remainder thrown upon a filter. If the fluid however is viscid, it will be useful to substitute water for the liquid poured off, to allow the precipitate to settle again, and then to decant and filter. Lastly, when the filtration is finished, the filter is to be carefully compressed between several folds of bibulous paper, till the paper ceases to be moistened, the precipitate is to be scraped off with a knife, dried on a bit of smooth paper at a temperature somewhat above 212°, † and then subjected to the test of reduction. These directions, of course, are given for the sake of those only who are not accustomed to chemical manipulations. For the benefit of the same persons I shall also describe the easiest and surest mode of performing the reduction.

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* When the arsenic is in small proportion, the previous solution of the sulphuretted hydrogen has appeared to me always requisite for its action to take place; the stream may be transmitted for five or ten minutes without any sensible effect. On the other hand, if the arsenical solution is strong, the first bubble of gas causes a precipitate.

† This is preferable to the usual mode of drying the precipitate on the filter itself.

The best substance for reducing arsenic on the small scale is the black flux. * If the quantity of matter be not considerable, the reduction should never be attempted but in a glass tube closed at one end. † It should not be above three inches long; its diameter may vary from an eighth to a fourth of an inch, according to the quantity of material. The matter should not fill above three-fourths of an inch of the length of the tube. It is introduced most easily, without soiling the tube, by placing it in a little triangular gutter of thin pasteboard, sliding this nearly to the bottom, and then knocking the sealed end gently on the table. The pasteboard being withdrawn, the sealed end of the tube is to be held (naked) in the flame of a spirit-lamp, or common blow-pipe lamp; but the former is preferable, as no blow-pipe is required, and the heat is never too violent. ‡ The open end of the tube should not be closed, as is generally recommended. The heat must be applied gently at first, otherwise the watery vapour disengaged may propel the material out of the tube. As fast as the moisture condenses on the upper part of the tube, it should be dried by a little roll of filtering paper. When the watery vapour has nearly ceased to be disengaged, a black vapour rises and condenses on the tube. This is charcoal, derived from the decomposition of the animal or vegetable matter, and carried along by the steam and gases. The arsenic does not begin to sublime till the discharge of carbonaceous vapour has nearly ceased; and it always condenses lower down, and within the shortest possible distance from that part of the tube which is exposed to the flame.

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* Almost all authors on chemistry and medical jurisprudence recommend, as an alternative, a mixture of charcoal powder and potass. But it is liable to serious objections. On the one hand, the charcoal powder, if minute, is raised in the tube by the current of gas and vapour disengaged, and forming a crust on the inside, obscures the sublimed metal, or may be mistaken for it. On the other hand, the disengagement of the water of the potass causes such an effervescence, that the matter froths up and fills the tube before any arsenic can be sublimed. Neither of these inconveniences attends the employment of the black flux. The charcoal of the black flux is not necessary in the process; and subcarbonate of potass might therefore answer as well, but it is seldom so dry.

† As a tube is not always to be had, various shifts have been recommended by authors. But it should be remembered, that these are only available in the hands of the experienced. A glass tube may be sealed by the heat of a spirit-lamp, or gas light, without the aid of the blow-pipe. If the operator is not accustomed to glass-blowing, he will easily succeed in closing it by heating it to redness, twisting one end between the finger and thumb till the cavity is obliterated, and then drawing it out a little, and cutting it with a file after it has cooled.

‡ The medical jurist is much indebted to Mr Phillips for suggesting the use of the spirit-lamp in this process. I need scarcely remind the unpractised operator, that he may easily convert a common oil-lamp into a spirit-lamp.

The true arsenical crust is known by the following physical characters. Its outer surface next the tube exactly resembles highly polished steel. Its inner surface (which is best seen by scratching the tube with a file at the lower margin of the crust, and snapping it across) is precisely like the fracture of fine steel, if the quantity is considerable; if it is very minute, it has a dull bluish-gray appearance; but before a microscope of four or five powers appears brilliant and crystalline like the fracture of steel. Occasionally, when very minute in quantity, it appears botryoidal and not brilliant, even before the microscope: in that case the part of the tube to which it is attached should be broken off, coarsely powdered, and heated anew in a tube of less diameter. It is scarcely possible for any one to mistake these characters, particularly if he has ever seen an arsenical crust before; * but to prevent all possibility of error, the analysis may be concluded with the following experiment, first suggested, I believe, by Orfila. The part of the tube to which the crust is attached being broken into fragments, is to be left for some hours in a watch-glass containing a dilute solution of the ammoniacal sulphate of copper, and covered to prevent rapid evaporation. In four or five hours the metallic crust will become grass-green; or, if very minute, it will be dissolved, and a brilliant grass-green crust formed on the surface of the liquid. The simple evaporation of the fluid will cause the formation of a crust on its surface, though no arsenic be immersed in it. But in that case its colour is pale azure-blue. The colour of the crust on the liquid is best seen by skimming it off with a bit of stiff white or blue paper.

The following appear to me the chief advantages which the method now recommended possesses over every other.

1. In point of simplicity, it is evidently much superior to that of Rose or Rapp; and, so far as my own trials go, it is equal, or even superior to them in delicacy. I have applied it to tea, coffee, broth, milk, porter, and port-wine; and I find, that from one quarter of a grain of the oxide, dissolved in four ounces and a half, or 8000 parts of each of these liquids, a sufficient precipitate may be procured for carrying on the subsequent process of reduction with perfect accuracy. † It is not long since the

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* Dr Paris says he has known a crust of charcoal mistaken for that of arsenic. I have myself known the same error committed; and a careless observer may readily commit it; for the outer surface of the carbonaceous crust is often very brilliant. But it is brown and internally dull, so that when examined attentively, it can never be mistaken for arsenic.

† The only one of these fluids which caused any embarrassment, was milk; for on heating it to expel the excess of sulphuretted hydrogen, a quantity of caseïn is thrown down, which neither the previous boiling nor the acetic acid can separate; and consequently the proportion of animal matter is too great for the ready per-

best authorities believed, that to show the test by reduction required three times that quantity even of the oxide of arsenic in a state of purity. *

2. Sulphuretted hydrogen, used with the precautions I have mentioned, never fails to throw down arsenic, even in the small proportion in which it existed in the fluids enumerated above. Since these fluids are as complicated and different from one another as can well be imagined, it is fair to infer that this rule has no exception.

3. At the very commencement of the process we procure a presumption of the presence or absence of arsenic. For sulphuretted hydrogen does not, like the three other precipitants, cause any precipitation with the unsophisticated fluids themselves. Hence, if the liquor remains undisturbed, after concentrating it, and subjecting it anew to the gas, the process may be abandoned, and we may rest assured that arsenic does not exist in such quantity as to be appreciable by any method of analysis.

4. It has a great advantage over the nitrate of silver used in the process of Dr Paris, by throwing down but a small quantity of organic matter with the precipitate. The quantity is too insignificant to impede materially the process of reduction.

Finally, the evidence it affords of the existence of arsenic is quite unimpeachable. † If a precipitate be procured with the

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formance of the subsequent process of reduction. To obviate this inconvenience, the heat applied to expel the gas in the case of milk, should not exceed 140 or 150°; or the gas may be allowed to escape gradually by exposure to the air for two or three days.

* Which was owing, I believe, to the mode of heating the tube. With the blowpipe or alcohol-lamp, I have several times succeeded in procuring a decided arsenical crust from a sixteenth of a grain of the oxide.

† A recent author suggests, that the test of reduction is liable to fallacy—from the possibility of an error as to the identity of the suspected matter. It is certainly proper to put the medical jurist on his guard. But if he is careful, such an error appears next to impossible.—Medico-legal authors have recently manifested a propensity to underrate the force of the evidence derived from the reduction of arsenic. (Paris & Fonblanque, II. 251. Beck. II. 219. Smith, 104.) The last author, in particular, says, " Perhaps all that the majority of medical practitioners could swear to, would be, that some metallic substance was encrusted on the glass. " I shall simply state what metals may be confounded with sublimed arsenic, and leave the reader to judge for himself what weight is due to these opinions. The only other volatilizable metals are zinc, tellurium, potassium, cadmium, and mercury. Now, zinc requires a full white heat for its sublimation; mercury sublimes in sparse white globules, which can never be mistaken for arsenic; and the three others, besides having a different appearance, are so exceedingly rare as to be altogether out of the question in medico-legal inquiries. Two other metals undergo a sort of spurious sublimation during their reduction, namely, antimony and bismuth. Minute particles of them are carried along with the carbonic acid gas, without proper vaporization, and then will condense in a crystalline mass. But this cannot be effected by the process I have described; at least I have never been able to procure any appearance of sublimate by reducing these metals in a small tube, under the full red heat of the common blowpipe.

sulphuretted hydrogen, and that precipitate yield, by the process of reduction, a crust possessing the physical characters and chemical property described above, it is quite impossible to draw any other conclusion, than that arsenic has been detected.

XI.

Documents relative to the History of the Malignant Puerperal Fever which prevailed in the Lying-in-Institution in Vienna, from the beginning of August to the middle of November 1819.

These documents are published in the Medical Annals of the Austrian States, first volume, new series, 1822, and consist of, 1*st*, A description of the disease and its progress, extracted from a Report to the government of Lower Austria by Dr Raimann, Director of the General Hospital of Vienna, and Professor of Clinical Medicine; 2*d*, History of the disease from the beginning of August, to the middle of November, by Dr Boër, Professor of Midwifery; 3*d*, Report of the appearances observed in the dissection of those who died, by Prosector Biermayer; 4*th*, Report of a Committee appointed to investigate the subject.

We have translated literally the two last documents; the last, as containing a summary of the whole observations made, and the preceding, as containing the most extensive and indisputably authenticated account of the organic pathological effects of this dreadful malady.

For understanding the relation of the various Hospitals and Institutions mentioned in these Reports, it is necessary to mention, that in the great General Hospital of Vienna, there is a separate Institution allotted for the teaching of midwifery, and that there are other lying-in wards in the hospital, not connected with the school. For distinction's sake, we have designated the former, to which the disease was almost confined, by the title of the Institution, or Lying-in Institution.—*Editors.*

Report of the Dissections of Women who died in Child-bed in the General Hospital of Vienna, from July 26th to the end of August 1819. By Prosector Dr Biermayer.

There were opened during this period 56 puerperal women.

Of this number were from the Lying-in Institution	-	38
From the Clinical Ward of Professor Raimann	-	3

From the division of the Lying-in Hospital of Dr Pellam	1
From the division of Dr Schiffner	11
From the division of Dr Festi	1
From the division of Dr Belletzky	2

Of this total number, three cases came into the hospital after having been delivered in private.

They were mostly between twenty and thirty years of age; three were eighteen, one nineteen, six above thirty, and two forty.

All were well formed; most of them of a full habit of body; only three emaciated. *

In all were observed many livid blotches over the whole body, and rapid putrefaction soon after death; in most of them immediately, so that in a few hours the bodies could scarcely be opened.

In all there was a bloody discharge from the mouth and nostrils.

In all the belly was more or less greatly distended, except in two, who had been delivered a considerable time before death.

The parts of generation were every where more or less swollen, discoloured, reddish, and foul; four were syphilitic.

Marks of blood-letting appeared in 13; of leeches on the abdomen in 7; of blisters alone in 17; and of both cupping and blisters in 12.

In the head, the organs were always turgid with blood; the ventricles generally contained more than the usual quantity of serum; in other respects there was nothing worthy of notice in reference to this disease.

In the trachea there was generally found a sanguineous fluid, and its internal surface was reddened.

The lungs were always in the greatest state of expansion, turgid with blood, frequently adhering or united by effused lymph to the pleura, which was generally, but not in all, slightly red.

In the cavity of the thorax and pericardium, there was invariably more than the usual quantity of bloody serum; the pericardium in no case morbidly changed, nor the heart externally, but its substance, without exception, more flaccid and tender than in the healthy state. Its internal surface, particularly the valves, chiefly of the right side, of a deep red, often of a black colour; the mass of blood generally fluid.

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* This fact is inconsistent with the opinion advanced by Prof. Boër, that want, hard labour and misery, were the causes, at least the predispository causes, of this disease.—*Editors.*

In the abdomen there were only two cases in which there was no unnatural fluid, *i. e.* in those cases which had been delivered a considerable time before death. In all the rest there was found from one to two quarts of turbid, very fetid fluid, mixed with portions of coagulated lymph, and sometimes purulent matter; the latter appearance was observed in those cases where powerful antiphlogistic means had been employed, and which had survived longer after delivery.

The peritoneum, omentum, and mesentery, exhibited, in five cases only, no appearance of redness; in the rest it was always somewhat more or less red, particularly towards the pelvis; and they were often agglutinated with the adjacent parts.

With the exception of two cases, the stomach and intestines were always much distended with air, and their external surface more or less red. *Lumbrici* frequently appeared in great numbers, not only in the small, but also in the large intestines, in the stomach, rectum; and, in one case, in the nostrils.

The liver and spleen were always similarly affected; they were much more pale, flabby, and tender than in their healthy state; easily broken down with the finger, similar to the degeneration of the uterus, and filled with somewhat fluid blood; These appearances were seen even in those two cases where the abdomen was not distended, nor contained fluid, and in which the intestines were healthy. The gall-bladder was always much filled with dark bile. The pancreas always healthy.

The kidneys, in most of the cases, were flabby and tender. The ureters always somewhat enlarged, and red. The urinary bladder always contracted.

The internal parts of generation were every where covered with yellow coagulated lymph, except in the two cases, in which no fluid was found in the abdomen; and in those cases in which strong antiphlogistic means were used, there was frequently a thick yellow purulent fluid often externally on the neck of the womb. The ovaria and Fallopian tubes were always more or less swollen, red and tender.

The uterus, in all cases little contracted, was more or less red externally, even in those where the delivery had taken place long before, and the abdomen was not otherwise in an unhealthy condition. The substance or body of the uterus was always flabby, tender, easily broken down by the finger; in two cases, full of small holes or cavities filled with stinking blood. In two cases, the uterus, on account of the tenderness of its substance, had burst, during delivery, at its neck; in the one case, the rupture was four inches in length; and, in the other, one inch and a half. The cavity of the uterus was found filled with fetid air several times, particularly in the syphilitic women. Its internal surface appeared generally covered with offensive cineritious ichor or mucus, only seldom with offensive viscid blood. Beneath this, it was always red, discoloured, often as if slightly eroded or ulcerated, the internal membrane very much eroded, and destroyed. Thus, in a woman of eighteen years of age, who had been delivered three weeks, in whom the abdomen was not distended, nor contained fluid, and there was nothing diseased in the abdominal cavity, except the liver and spleen being flabby and tender, the uterus was entirely ulcerated on its internal surface, like the internal surface of one of the large intestines, when covered with ulcerations. She died on the twenty-ninth day. On the fundus of the uterus, there generally remained particles of the placenta and *membrana decidua*, in the form of putrid shreds. The colour of the cervix of the uterus was constantly darker than the rest; and it always was darkest, and penetrated into its substance, at the mouth of the womb, which was likewise much dilated, and often as if destroyed with loss of substance, often cracked, as if rent. The vagina was generally preternaturally red.

Such were the facts carefully abstracted from the whole fifty-six dissections.

Report of the Committee appointed for a more Minute Investigation of this Disease.

By a government order of the 27th August, a Committee, consisting of Baron Von Matoschek, Vice-Director, Professor Hartmann, and the provisional Hospital Director, Raimann, was appointed to inquire and report without delay;—

1. Whether an epidemic disease and great mortality has occurred among the women delivered in the Lying-in Institution of the Midwifery School in the General Hospital of Vienna? and, if so,

2. Whether the epidemic and great mortality occurred only among those delivered in the said Midwifery School, or also in the other Midwifery departments of the public Hospital?

3. To what causes and circumstances the frequent disease and great mortality among those delivered might be ascribed?

4. What is to be done to check both?

Conformably to this order, the Committee met on the 28th August in the Midwifery School, and terminated their inquiries on the 31st of the same month. The Committee witnessed the dreadful spectacle of a disease which hurried inevitably to the tomb, in the prime of life, those seized with it. On the very first day they saw five corpses, and scarcely as many patients. They had an opportunity of examining it in its commencement, *acmé*, and last stage.

The beginning and progress of the disease, reckoning only from the period when it assumes the appearance of a febrile affection, (for according to the assertion of Professor Boër, the accession of the fever is always preceded by marked changes in the whole system, and particularly in the uterus), indicate clearly an inflammatory state. The symptoms were such, that the inflammation of the uterus and peritoneum, combined with high fever, could not be mistaken. The lochia disappeared either immediately, or in a few hours; and the mammæ were found empty of milk, loose, and flabby. But the most alarming circumstance was, that while the disease was yet, in all appearances, a recent acute inflammation, Dr Boër, on examination with the finger, already discovered, in the mouth of the uterus, marks of gangrenous disorganization, which were rendered evident even to the bystanders, by the putrid smell of the finger.

Those patients who were in the last stage presented all the symptoms of effusion into the thorax and abdomen, extreme debility of the powers of life, and gradual dissolution. This view of the disease was in every respect confirmed by appearances found in the bodies of the deceased, seven of which were examined in the presence of the committee. The very rapid putrefaction of the body taking place in a few hours after death —the dissolved state of the blood—the strikingly soft and tender state of the whole bowels, the heart, lungs, liver, spleen, kidneys, and particularly of the uterus,—indicated a colliquative putrescent condition of the whole system, induced by the disease. There were found, moreover, in the cavities of the breast and abdomen, effusions of brownish serum, generally mixed with coagulated lymph. On the peritoneum and ovaria were traces of inflammation, more or less evident, and the internal surface of the uterus was destroyed by gangrene, which was, in every instance, most strongly marked at the *os uteri;* so that it appeared as if it spread from this point as from a focus. Exactly the same appearances were found, according to the unanimous testimony of Professors Boër and Biermayer, in all the dead bodies, fifty-six of which were opened from the 26th July, the day the disease broke out, till the 31st August, when the committee terminated the investigation, as the extract from the register of dissections shows.

The treatment of the disease, according to the report of Professor Boër and his assistant, confirmed by Dr Raimann, had hitherto been chiefly antiphlogistic. They employed in most cases, immediately on the commencement of the disease, repeated venesections, the application of leeches, emollient cataplasms, emollient clysters; at a later period, blisters, with the corresponding internal remedies; in some cases calomel and other celebrated remedies; and in some, where gastric affections at first predominated, emetics and other appropriate remedies were given. On the commencement of debility, they had recourse to diffusible stimulants, but always with the same unfortunate result; the only difference being, that some were carried off by the violence of the disease very quickly, even in a few days, and one in six hours, others more slowly. The chief physicians, Schiffner, Festi, Belletzky, and Director Raimann, were not less unfortunate in the treatment of those pregnant women whom they took out of the Institution into their respective divisions of the Hospital. Notwithstanding the meritorious and scientific exertions of these able practitioners, all of their patients died.

There were delivered in the Institution in July, 187; in August, 231; in all, 418. Forty-three of these died from the commencement of the epidemic, *i. e.* July 26th, consequently about one in five; while in the lying-in wards of the public hospital, of about forty women who were delivered in them, only one was seized and carried off by this epidemic; from which it appears that this excessive mortality was confined to the Lying-in Institution.

From all that the committee learned, the causes of this mortality could not be attributed to any want of attention, or of careful treatment and nursing, but chiefly to the malignant nature of the disease itself, highly inimical to life, which, at the period when, in consequence of marked symptoms, it became an object of observation and treatment by the physician, had already produced incurable disorganization in one of the most important organs of the female system; for, according to all the observations of Dr Boër, the mouth of the uterus exhibits, even on the breaking out of the fever, undeniable traces of putrescency. In this state of the matter, it is of the utmost importance to detect the causes of this dreadful malady, so that we may use the most powerful prophylactic means to counteract them, as all attempts at cure have hitherto completely failed. But even on this point the committee are involved in great difficulties. In the Institution they found nothing obvious, at least to the senses, that could be considered as the cause of so desolating a disease. According to the opinion of Dr Boër, the pregnant women brought the germ of the disease with them into the Institution, and the process of delivery was only the occasion of its perfect development; for by far the greater number of these women, previous to their admission into the Institution, felt many symptoms of a diseased state, generally burning heat in the abdomen, and in the region of the pubes, combined with great uneasiness, and frequent slight shiverings, distressing pain in both shoulders; and the greater number brought with them into the hospital, even erythematic spots on the hands and feet, chiefly upon the joints. Most of those, 40 in number, who fell a sacrifice to this disease, were persons who had experienced, during their pregnancy, many adverse circumstances, and came into the Institution only 6, 12, or 24 hours before their delivery; and in whom, on the 1st, 2d, and at latest, on the 3d day, indications of the disease already appeared; some even died in from 12 to 24 hours after the first accession of fever, one in six hours after delivery, one during the labour pains, and in one the uterus was burst during a slight bearing down pain, by which the fœtus escaped into the abdomen. These three bodies showed on dissection, the previously diseased state of the uterus. Of those admitted several weeks before delivery, thirteen only were seized with this fatal disease. It may easily be conceived, that in pregnant women who are badly nourished, undergo hard labour, vexation and care, and live not unfrequently in low, damp, close dwellings, under the influence of excessive heat and continued moisture, such as occurred this summer, a gastric and scorbutic diathesis may be developed; nor is it less conceivable, that if inflammation occur in such a diathesis, it must have the greatest tendency to gangrenous disorganization, particularly if scarlet fever and erysipelatous eruptions prevail, as this summer evinced. * But shall a disease, which in every individual seized with it, terminates in gangrene of the organs chiefly affected, and in destruction of the whole organization, which appears as a highly malignant, we may say pestilential, fever, not also at last generate contagious matter, since it is universally allowed that gangrenous and putrid fever very readily produces contagious matter?

This question was brought under consideration by the committee; but Professor Boër declared, that up to this period he had never observed any trace of contagion.

The Committee very readily admit, that the opinion of such a man as Professor Boër, who had so many opportunities of

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* That the mode of life had no concern in the evolution of this disease, was put beyond a doubt by our unfortunate experience in this city, when it spared neither high nor low, rich nor poor, those delivered in hospitals, in the city, nor in the environs, neither the gay and dissipated, nor the most retired and domestic. Editors.

observing this disease, and has so admirably described it, merits the first attention in regard to its nature and origin. But they still retain reasonable doubts of its non-contagious nature, on the following grounds.

1. If this disease were wholly the consequence of an indigent, hard and laborious mode of living, combined with the influence of the *constitutio annua*, Why does it not more frequently occur out of the Institution, in married women of the poorer classes of society who are exposed to the influence of the same prejudicial circumstances?

In the bills of mortality of the city of Vienna, there were in the course of this month few cases of death among parturient women.

2. Why did the disease prevail with such violence in the Institution, while, during the month of August, in the detached wards of the public hospital for lying-in women, received as boarders, scarcely a trace of it was seen? In the Institution, during the course of last month, one in five parturient women died, while in the other wards, of forty delivered, only one was seized and carried off. It is true, that into the latter, there were admitted only women who paid, and we may suppose therefore that they lived in less distressed circumstances, and that in them the disposition to this disease was not so easily unfolded. But the Institution had also a certain number of patients who paid, and in these the same proportion of disease and deaths took place during August, as in those that did not pay; of thirty paying patients six died, or one in five, being the same proportion as in the gratuitous patients.

3. A fact related by Dr Schiffner, who superintended the third medical division of the public hospital, could not fail to excite the attention of the committee. This physician received eight puerperal patients from the city into his division, most of whom had left the lying-in hospital only a few days before. Of these eight there died one, in whom there had been formed, when in her lodgings, a deposition of milk in the right thigh; (Plegmatia dolens? *Edit.*) two have already left the hospital cured; and two were still in danger on the 31st August; the remaining three, although malignant symptoms accompanied their disease, were convalescent; and yet this physician lost all the puerperal women who were brought directly out of the Institution into his division.

4. Has not this disease, in respect of its nature, malignity, and the obvious changes which it produces in the organs affected, a striking analogy with other diseases evidently contagious, *i. e.* hospital gangrene, typhus, and even the exotic pestilential diseases?

But if from all these circumstances suspicion at least must arise that the malignant puerperal fever in the Institution, although not produced in every case by contagion, was multiplied and as if domiciliated in it by contagion, it becomes indispensably necessary to do all that circumspection demands in so important a case by employing in the Institution those measures by which the spreading of such a contagious disease may be powerfully checked.

The committee have in the mean time ordered, together with the measures known to every physician, that all newly admitted patients, as well as those previously admitted, in whom the precursory symptoms of puerperal fever appear, be put into a particular lying-in ward appropriated for them; and that the greatest precaution be also observed in examination by the touch, in order that the disease may not be communicated to those in health from patients in whom are discovered marks of putrefaction at the mouth of the uterus.

In cases brought on by misery of life and atmospherical influence, the Institution can do nothing effectual in the way of prophylaxis, as the Institution has no cognizance of the pregnant women at the period when the disease becomes developed in their system.

It is more difficult to point out an effectual method of treatment when the disease is present, as its malignant and complicated nature is evinced on the one hand by inflammatory irritation, and on the other by simultaneous putrid solution, excessive propensity to effusion in the pectoral and abdominal cavities; and its extremely rapid course in so many cases, opposes so great impediments to every attempt at cure. In each individual case this must be left to the judgment of the experienced and skilful practitioners who superintend this Institution.

BARON VON MATOSCHEK.

Vienna, 6th September, 1819.

XII.

Cases by JOSEPH SWAN, Surgeon, Lincoln.

Cases of Calculi in the Bladder, and Diseases of the Kidneys.

MR C., æt. 74, had enjoyed very good health until the beginning of September 1820, when he became feverish, and had a disordered state of the digestive organs. A short time after this he passed several calculi of different sorts. He began to suffer irritation in his bladder, and frequently passed red sand, and calculi of different sorts. The urine was for the most part clear. He continued to have more or less of this irritation, and his flesh gradually wasted. For several months he had not decided symptoms of stone in the bladder. About September 8th, 1821, there appeared inflammation near the anus, and this continued to increase for several days. He became insensible on the evening of the 14th, and appeared as if dying. I made an opening on the right side of the anus, and a quantity of very putrid matter was discharged. After this he recovered in some degree. He then began to get worse, and died on the 3d of October.

About six weeks before he died, a large tumour about the size of a child's head was discovered on the left side, extending from the situation of the kidney forward into the abdomen. It never gave pain, even on pressure, nor did he ever complain of pain in his back. His bowels were always very open; but a year before he died, they became confined, and always required aperient medicines.

With respect to the treatment of his complaints, I need say very little. He experienced much relief from soda, henbane, the black drop, and clysters.

Many years ago his horse threw him on the pummel of the saddle, and for some time he passed with his urine something resembling soft mortar. It was then thought he had a stone in his bladder; but Mr Pott ascertained, by sounding, that it was not so, and the symptoms left him, and did not return.

Examination.—On opening the abdomen, the spleen was found much enlarged. The left kidney was very large; and on being divided, was found to have a large cavity containing putrid matter. In the infundibula there was some red sand, exactly like that usually observed in the urine. After the matter was removed, the kidney weighed two pounds and a half avoirdupois. The right kidney was sound, and weighed seven ounces. The bladder contained four calculi, and was sound, except a slight enlargement of the prostate gland. There was a stricture in the rectum. The matter discharged from the abscess resembled that contained in the kidney, but I could not exactly ascertain whether these parts communicated.

CASE II.—WILLIAM RUSSEL, æt. about 40, suffered very much from stone in the bladder, and wished to have the operation performed. He appeared to be in perfect health. A day was fixed for the operation; but, early in the morning, he was seized with pain in his back, and rigors, and his pulse was 130.

He continued to have fever, with a great disturbance of the digestive organs, and became quite jaundiced. His body was emaciated; but he appeared to die more from the constitutional irritation than the emaciation.

For about eight years he had frequent attacks of pain in the back, about the region of the kidneys, which were attended by a great discharge of blood from the bladder. Riding always brought them on; and he was invariably relieved by bleeding from the arm.

Examination.—On opening the abdomen, every part was much loaded with fat. The liver was sound, and the gall-bladder distended with bile. The pancreas was enlarged, and had a scirrhous hardness; it pressed on the termination of the gall-duct, and caused an interruption to the free course of the bile. Both kidneys were enlarged and soft, and contained several bags, which had the appearance of hydatids. Some purulent matter was observed in the division of the kidneys. The disease was in the greatest degree in the left; and its ureter, near the termination in the bladder, contained a small calculus. The bladder was sound, but contained three calculi.

Case of Rupture of the Jejunum.

John Copaldish, æt. about 70, was kicked by a horse just above the navel, on the 24th of April 1821, at one *p. m.* He was very faint for some time, and in extreme pain. He was bled, and had his body fomented; he took an opiate, and had a clyster. He was in great pain always at the spot where he was injured. There was a little discoloration, as from a slight bruise, but there was not the least wound. The abdomen became tense about two hours after the accident, and so tender as not to bear being touched. He died at ten *a. m.* on the 25th. He was able to retain some gruel and other mild things on his stomach, and he never vomited after the accident. The clyster purged him well, and passed plenty of urine.

Examination.—There was a small opening in the lower part of the jejunum, and the lips of the wound were everted; the whole of the small intestines near the injured part were inflamed, so that coagulable lymph was effused, by which several folds were glued together. The villous coat was not inflamed. The whole peritoneum was becoming inflamed; but the omentum was in a high state of inflammation, and adhered to the intestines. There was a large quantity of bloody serum in the abdomen, but very little feculent matter had escaped. On two portions of the intestine there was a black patch of the size of a shilling, which was only an ecchymosis.

Case of Chorea.

Master Toynbee, æt. 9 years, had for the last three months been losing his flesh and spirits, and his arms had become very weak. About the end of September 1822, he was seized with chorea, which affected almost every muscle of the body. He took submuriate of mercury and purging medicines, as his bowels were very much constipated, and his stools black. He complained much of the back of his head. His appetite was various. The feces resumed a proper appearance, but he still continued costive; and the chorea continued unabated. On the 17th of October a blister was applied to the lower part of the back, which immediately diminished the motions very much; and after the application of a second blister about the 24th, they entirely ceased. He remained very weak, and his left arm and leg were nearly paralytic, and he had very great pain on attempting to move them. There was not the least swelling in either limb, and the pain seemed very much like that of tic douloureux; and pressure on their nerves produced great pain. He took ten grains of carbonate of iron, and ten grains of powdered bark, three times a day for a fortnight. Under the use of these medicines the pains and paralytic affection gradually left him, and he has regained his perfect health.

XIII.

Case of Internal Hernia occasioned by an unusual Cause.

By T. M. Greenhow, Surgeon.

Ellen Jordan, æt. 31, was delivered April 14th, of her seventh child. On the 24th I was requested to visit her, when I found her complaining of severe colicky pain with sickness, and slight soreness of the abdomen; which symptoms were entirely removed by active purgatives. Repeated doses of medicine were afterwards required to keep up the action of the bowels, but her recovery went on favourably in other respects. On the 3d of May she had a return of the colic which yielded to the former treatment. May 10th, at 3 *p. m.*, I was again requested to visit her; she had been rather unwell on the preceding day, but at 10 o'clock on that morning had had a distinct shivering fit, with excessive pain and tenderness of the abdomen, headach and unremitting vomiting. Bleeding, purgatives, glisters and warm baths were had recourse to in vain. The vomiting continued until within a few hours of her death; and no evacuation from the bowels took place. She died at 2 *p. m.* on the 11th, only 28 hours after the occurrence of the last acute train of symptoms.

Appearances on dissection 18 hours after death. There was considerable tumefaction of the abdomen, and putrefaction had already commenced. The small intestines were intensely inflamed, and in one part approached to a state of gangrene; their convolutions adhered firmly together. The omentum was much thickened and inflamed, and adhered very firmly to the fundus uteri,—indeed so firmly as to be quite inseparable without the use of the knife; it also adhered to the peritoneal covering of the brim of the pelvis, and embraced the colon and ilium so closely, as to insulate the cæcum, which was enormously distended, and formed a large strangulated internal hernia. The uterus had regained its natural dimensions; about two pints of bloody serum were contained in the cavity of the abdomen.

On inquiring into the history of this woman, I found that after the birth of each of her four last children, she had had attacks of peritoneal inflammation, as well as repeated threatenings, at least, of it during her last pregnancy. For some years she had felt a painful dragging sensation in the right iliac region, which induced her, in walking, to lean a little to that side, and was attended with a sensation of numbness in the right leg. During the same period she had occasional colics, and her bowels required the frequent assistance of purgatives. These circumstances render it probable that the adhesions between the omentum and fundus uteri, which constituted so remarkable a feature in this case, had existed (in part at least) for a considerable length of time.

The mechanism of the hernia is not difficult to explain, although at first I felt considerably at a loss entirely to comprehend it. The omentum must have formed its adhesions to the fundus uteri, when the womb was distended by its contents during pregnancy, and its upper part was naturally brought into contact with the omentum. After delivery, the uterus, in contracting to its original dimensions, had dragged the omentum with it into the cavity of the pelvis. The omentum, thus displaced, formed a sort of bridge over the colon and ilium, and separated the cæcum from the rest of the intestinal canal. At every successive attack of inflammation its adhesions had increased, and it had become more dense in its texture, until the strangulation of the cæcum was completed by it.

The immediate exciting causes of the last attack, appeared to have been partly cold and partly indigestible food. On the 8th she had sat in her room while the floor remained wet after having been scoured, and had thrown aside a flannel bandage which had been constantly worn round the abdomen since her delivery; and on the same day she had eaten heartily of roasted sheep's heart at dinner. It is needless to offer any comment on the combined influence of these circumstances.

At each of my visits to this patient, her symptoms so nearly resembled those of strangulated hernia, that I was induced to make particular inquiries respecting the existence of any such tumour. And on dissection I was prepared to meet with some decided obstruction, probably, as I suggested to my pupils, a stricture or intussusception. Although I could not anticipate so unusual a state of disease, it was still very satisfactory to me to have the principle on which my views of its nature were founded so completely confirmed.

Newcastle upon Tyne, May 12, 1824.

PART II.

CRITICAL ANALYSIS.

I.

Report from the Select Committee on the State of the Penitentiary at Milbank. Ordered, by the House of Commons, to be Printed, 8th July 1823.

THE investigation which has recently been instituted into the condition of the prisoners confined in the General Penitentiary at Milbank, has afforded several curious points for the consideration of the Physician and the Student of Medical Police. It has demonstrated to the world the discordance of medical testimony; it has shown how opposite are the opinions on the causes and best mode of combating diseases; it has exhibited the glorious uncertainty of medicine; it has revealed the nature of those heterogeneous materials of which managers of such institutions consist; and it has finally proved, most clearly, the gross credulity of those who form opinions and adopt measures on the testimony of others. It is not our wish, on the present occasion, either to eulogise one party at the expense of the other, or even to preserve an ambiguous neutrality, by declaring the errors of both. But we think there is scarcely an institution in the kingdom which appears to have been so well provided with managers of the most enlightened minds and the most zealous benevolence, and yet the recent history of which exhibits such deplorable proofs of its true interests being utterly misunderstood, as are contained in the Minutes of Evidence before us. What are the causes of these errors, it may be difficult, or even impossible, to discover; their existence is undeniable, and indeed is admitted in the admirable and perspicuous Report by which the Minutes are preceded. Without entering, however, into any speculation on this subject, which is doubtless in this place premature, we shall proceed, without delay, to take a view of the principal facts which the inquiry has brought to light, or which have been otherwise proved, accompanied with such observations as they may naturally suggest.

For the sake of those who are unacquainted with the locality, uses, and nature of the Institution of which we are now to speak, we shall merely say, that the original object of the General Penitentiary was the reception of those offenders,—who, though not hardened by the habitual violation of the laws, had yet committed crimes for which transportation has been usually inflicted,—and who, by confinement in such a place, might, if incapable of being reclaimed to virtue, at least not be so wholly corrupted and debased by vicious associates, as too often is the necessary result of the other mode of punishment. With this view, in pursuance of the recommendation of a Committee of the House of Commons in 1811, subsequently repeated in 1812, ground was selected at Milbank, in the parish of St John's, the Evangelist, Westminster, close on the Thames; the foundation was laid; and in June 1816, the quadrangle, which was then completed, was opened for the reception of female convicts. In the course of 1817, as other parts of the building were completed, 103 male convicts were admitted. In 1818 and 1819, the admissions were less numerous; but during the three subsequent years of 1820, 1821, and 1822, they amounted to 440 men and 254 women. When visited by the Committee of Inquiry in May 1823, it contained 566 male, and 303 female convicts; or, in all, 869 of both sexes. Its internal arrangement was conducted upon the most approved plans. The part of the Institution assigned to the convicts consists of six pentagonal buildings, four of which contain male convicts, and two female. Each of the four male pentagons, Nos. 1, 2, 5, 6, has a task-master and an assistant, and is divided into five wards, with a turnkey and machine-keeper or miller to each. The two female pentagons, Nos. 3 and 4, have a task-mistress and an assistant, and are divided into six wards, one of which is a laundry for cleansing the clothes of all the prisoners, both male and female. In the centre of these six pentagonal buildings, a hexagon contains the apartments allotted to the governor, task-masters, and other officers of the Institution. The Institution contains also three kitchens and three infirmaries, two of which are attached to the male, and one to the female pentagons. Thus arranged, the Institution appears, on the whole, to have been very healthy. During the three years subsequent to its first opening, 1817, 1818, and 1819, three men died, and seven women. In 1820, when the Penitentiary contained 427 persons, five deaths occurred among the men, and seven among the women. In 1821, seventeen persons died out of a population of 636; and in 1822, only twenty-two deaths took place among 745 persons, which is only about one in thirty-four; so that the prison may be regarded as tolerably healthy.

In the minutes of evidence before us, each witness is examined successively on all the points relating to the health of the prisoners, internal economy of the Penitentiary, causes of disease in general, and treatment, and, in short, on most of those subjects which are connected either directly or collaterally with the management of bodies of men, especially when in a state of confinement. We shall make no apology for departing at once from the arrangement suggested by this mode of examination, and select those points which we conceive most deserving of attention, in the order most suitable to brevity and perspicuity. With this view, almost every thing which it is proper to know of the present investigation, may be referred to one or other of the following general heads;—1*st*, the nature, causes and treatment of the disease or diseases which appeared among the penitents during the winter of 1822–1823; 2*d*, the nature and justice of the charges exhibited against Dr Hutchison, the medical superintendent of the Institution; and 3*d*, the conduct of the managing committee in general, and particularly their treatment of Dr Hutchison. Though the evidence embraces several other subjects beside, we presume these to be the principal which can properly claim our attention on the present occasion; and we shall, in our account of the testimonies of the various witnesses, endeavour to restrict our observations as closely as possible to their consideration.

I. In the investigation of the first part of our subject,—the nature of the prevailing disease,—we are prepared to expect some astonishment at the expression of our doubts, that the disease was uniformly the same throughout the prison, and that genuine sea-scurvy affected its inmates to the frightful extent to which it was represented to prevail. It may be regarded as the very affectation of scepticism, when so many physicians and surgeons of the navy have pronounced on the disease, to request the evidence on which their opinion is founded; and we doubt not that we shall incur the charge of arbitrary and wanton infidelity in hesitating, for a single moment, to admit the assertions of the medical witnesses in their fullest extent. Not thus, however, are we wont to form our opinions, nor thus slightly can we admit an inference which involves considerations of much importance. It may not perhaps be entirely superfluous to bestow some attention on the grounds on which the disease in the Penitentiary has been declared to be sea-scurvy;—nor quite idle to inquire into the nature of that malady, which has been said to arise in a situation where it was certainly not expected, and to be cured by a remedy which all former experience had found to be not only useless, but injurious. If such inquiry shall prove the disease of the Penitentiary to be what it is alleged, our subsequent inferences will be formed with greater security, and we shall both more willingly and more rationally assent to those modifications of our medical tenets which these inferences inculcate;—if, on the other hand, it appear to be something different from what it has been stated and believed to be, not only will our inquiry not be unprofitable, but we shall undoubtedly be enabled to explain several circumstances which it is difficult to comprehend upon any other principle.

The first witnesses examined are Drs Peter Mark Roget and Peter Mere Latham, who had been requested, in consequence of the sickliness of the penitents, to visit the Penitentiary on the 1st of March, and report on the condition of the patients. The following extract from their Report of the 5th April will put our readers in possession of the state of health or sickness throughout the prison at the time to which we allude.

"Between the 14th of February and the 1st of March, no less than forty-eight prisoners came into the infirmaries, affected chiefly with diarrhœa and dysentery. The diarrhœa and dysentery were of a peculiar kind, and were suspected to have a connexion with the scorbutic disease. At this time, also, all these various affections were found spreading extensively, but in different degrees of severity, throughout the prison.

"On the 28th of February our assistance was called for; and having learned the facts already detailed, we began our examination of the prison and the infirmaries on the 1st of March. We found the prevailing disease to be the same with that which is known by the name of *sea scurvy*, and which is characterized by livid spots, or blotches of the skin, especially on the lower extremities. Conjoined with the scurvy, in almost every case, there was diarrhœa or dysentery. There were, indeed, a few instances of scurvy without disorder of the bowels; and moreover, numerous instances occurred of diarrhœa and dysentery, where no marks of scurvy had appeared. But still, whether the scurvy subsisted alone, or the diarrhœa or dysentery subsisted alone, or whether they were conjoined in the same individuals, there was found in all those who suffered from either, or from both, the same constitutional derangement, denoted by a sallow countenance, an impaired digestion, diminished muscular strength, a feeble circulation, various degrees of nervous affection, as tremors, cramps, or spasms, and various degrees of mental despondency.

" These facts seem to lead directly to the belief, that the diarrhœa and dysentery and scurvy had their origin in the same morbid state of the constitution. In this belief we were more and more confirmed by further observation; and we soon had the means of determining with certainty, that they, in reality, constituted one and the same disease. We examined, by dissection, the bodies of two prisoners who died dysenteric, and found, in various parts of the intestines, the morbid appearances called, in medical language, *ecchymoses;* that is, spots of the same kind as those which on the skin constitute scurvy. We found, in fact, an absolute scurvy of the bowels, of which the diarrhœa or dysentery was only a symptom and consequence." p. 388.

With this statement the examination of the witnesses in general accords. Dr Roget states that he was called to the Penitentiary on the 1st March, by a request from the committee; that on the same day, Dr Latham and he visited the infirmaries, and found a large proportion of patients—44 males and 66 females—affected with diarrhœas and dysenteries, and also with scurvy. On the 3d March, they proceeded to examine the prison generally, found it to contain between 850 and 860 persons, about one-third of whom had marks of scurvy on their legs, which, from the statements of the patients themselves, had been there since November 1822; and they learned from the matron, from Mr Pratt the surgeon, and from other officers of the Institution, that the health of the prisoners, especially the females, was broken so early as autumn 1822; but that it was the end of February 1823 before the patients began to crowd into the hospital numerously. At present (the day before examination, May 15), there are 94 men and 131 women labouring under symptoms of diarrhœa, of whom 48 men and 87 women are improving, while 69 of both sexes, who were ill, are now restored, but are retained in hospital for fear of relapse. Dr Roget, further examined, states, that scurvy makes its appearance on the legs; and he sees no reason to alter the opinion formerly given of the nature of the disease, unless that it was more severe than they had at first thought it; that it is now milder, and is chiefly in the form of diarrhœa.

Dr Peter Mere Latham states, in explanation, that the number 448 reported in the Appendix, refers to the disease under all its symptoms, or includes both cases of scurvy and of diarrhœa; but that 225 is the whole number of those now affected with diarrhœa;—which is, at this time, (May 16th, 1823), the only disease in the Penitentiary, scurvy having disappeared. In the extract given above, the appearances disclosed by the dissection of the two dysenteric patients are mentioned as ecchymoses or scorbutic spots in the intestines.

When Dr Roget is interrogated whether the medical superintendent communicated such facts as went to establish the same conclusion,—he answers, that he did not, because there had been no dissections previous to the attendance of Drs Latham and Roget; and that they ascertained by these dissections facts which had been only presumed as to the connexion of the cutaneous spots and the condition of the intestines. When Dr Latham is afterwards examined on the probability of the disease breaking down, or disappearing, he states, that the relapsed cases are less severe than formerly,—that they were not the subject of apprehension,—and that, at the end of last week, when he went round the infirmaries for the purpose of ascertaining the number of new cases, found that they amounted to no more than eight; has seen single and occasional cases only of scurvy as they occur in the London hospitals; believes there is no dictinction between sea and land-scurvy, and regards the former as an amphibious disease. On a subsequent examination at the Penitentiary on the 2d June, the same witnesses state, that the appearances of scurvy have very generally subsided; that the cases of diarrhœa, though more numerous, are milder in their character; and that the severity of the sickness is considerably diminished. On the 12th of the same month, their returns show that 232 persons of both sexes were still labouring under diarrhœa, 124 of whom were improving, and that 52 persons were affected with other diseases not mentioned, while 170 are stated to be well, but retained under medical treatment to prevent recurrence of the symptoms.

Sir Gilbert Blane was four years physician to the fleet in the West Indies, from 1779 to 1783, when from thirty to forty sail of the line were under his medical superintendance;—states, in the most decided terms, that what is called land-scurvy has not the least rememblance to sea-survy;—what is called *scurvy* in common language is a cutaneous disease, and derives its name from scurf;—sea-scurvy, (*scorbutus*), on the contrary, is named from a Saxon word, (*scharbocck*), meaning a sore mouth. Land-scurvy does not affect the intestines, and admits, that if the disease in question betrays this tendency, it is to be regarded as a proof that it partakes of the nature of sea-scurvy. Knows the disease of the Penitentiary by hearsay only, but from the description doubts not that it is sea-scurvy; admits, that, in persons in the last stage of sea-scurvy, pressure on the lower limbs leaves a complete wound of the part, as if it had been putty; never heard that this was the case in the Penitentiary; but if the disease were sea-scurvy, it shows itself first by a sore mouth, and then by spots in the legs.

At a subsequent examination on the 27th May, after visiting the Penitentiary, Sir G. Blane states, that he found the sea-scurvy in a slight form, but well marked and without ambiguity, prevailing to a great extent or among a great proportion of persons. Is uncertain whether he visited all the pentagons, but inspected about thirty male and female prisoners in the general cells of the house, and who were ranked among the healthy class, —at least, is not aware that any of them were under medical treatment. Their general health was good; their only complaint was a distant characteristic of sea-scurvy, consisting in a peculiarly rough appearance of the skin with small brown spots like flea-bites, and some degree of swelling and hardness of the muscles, hardness and inflammation of the ankles, and about one-half of them had sore gums. We shall afterwards find that this last symptom is ascribed by Dr Latham to the use of mercury, and is not part of the disease. (p. 103.) These symptoms, Sir G. continues to state, are not accompanied with suffering deserving the name of pain, but with much lassitude, great anguish and uneasiness from depression of spirits, and sometimess shortness of breath. Has seen scurvy of exactly the same nature ashore, in the garrison at Gibraltar during a long protracted siege, in 1780; during forty years practice in London never saw more than six cases of sea-scurvy, one of which was from bad food at sea, the other five from predisposition of the individual constitutions. Regards the disease of the Penitentiary as on the decline; derives these conclusions from examination of thirty persons who were specified as those longest in confinement and most affected, and found about two-thirds of them in the state described; knows that these persons were not under medical treatment; that they had been at the worst six weeks before, and were now convalescent.

At a further examination on the 18th June, Sir G. stated that he had visited the Penitentiary on the 17th, when he found a considerable diminution of the scorbutic symptoms, and presented the following statement of persons still affected with disease, or under treatment.

"Number of Persons not under Medical Treatment, examined by Sir Gilbert Blane, at the National Penitentiary, the 17th of June 1823.

Men - -	91
Women -	58
Total -	149

Of these, three were affected with symptoms of scurvy, namely, discoloration and roughness of the skin of the legs; induration of their fleshy parts; gums red, swollen, or ulcerated; all apparently convalescent, but some of them complaining still of pains in their extremities; preternatural languor and debility—

Men	19	
Women	10	
Total	29	nearly one-fifth of the whole.

Exclusive of these, there were some who had their gums affected without their legs being affected, as follows:

Men	11
Women	13
Total	24

Of these, some were ascertained to have undergone a mercurial treatment, in a recent bowel complaint; in others it was a symptom of scurvy.

"As I have found that some of my former evidence had been misunderstood, I have also put in writing what remarks occur to me, which I will read. I was informed that there were 460 persons in the infirmaries, or under medical treatment, of whom 400 were affected with bowel complaints.

"I have no doubt of this bowel complaint being one of the forms of scurvy; for, 1*st*, It obviously owed its existence to the same causes as the others, the chief of which was scanty diet; *2dly*, It has been in many cases attended with discharges of blood, to which scorbutic affections are extremely prone, from the great tenderness and weakness of the extreme vessels; *3dly*, The state of the intestines, on examination by dissection, are found in a state analogous to the external parts affected by the same disease, namely, the effusion of blood under the surface, and a great propensity to bleeding.

"It must be acknowledged, that this early appearance of diarrhœa, dysentery, and inward hemorrhage in this disease, is much more frequent in that which appears at land than that which occurs at sea. That which is met with at sea seldom appears till the last stage of it. Those authors who describe the former, make much more mention of flux and hemorrhage from the bowels, also of bleedings from the nose, than those who treat of it as it appears at sea; it is natural enough to suppose that there should be some difference in those cases which arise from scanty *fresh* diet, from those which arise from scanty *salt* diet." pp. 193, 194.

The witness, whose evidence our arrangement requires us next to notice, is Alexander Copland Hutchison, M. D., principal medical superintendent to the Penitentiary for seven years. Dr H. states that he first became acquainted with the presence of scurvy among the penitents about the 11th, 12th, or 13th January 1823, and does not remember that any complaint was made in November 1822. The symptoms first showed themselves in the person of David Jones, who, according to the notes of Dr H., taken in January and February, was admitted into the infirmary on the 2d of January with a pain of the side, for which he was ordered the blue pill. Some days after he complained of pain or stiffness of the left leg, which Mr Pratt the surgeon said was nothing. At the next visit, when he still complained, Dr H. examined the leg, recognised the disease to be scurvy, and ordered the man to have permanent full diet and the medicine to be continued. About this time, which was now the end of January, Dr Hutchison was requested to see an ecchymosed blotch on the thigh of Mary Ann Davison, a girl of 18, who had for months been labouring under symptoms of consumption; and for whom nutritious diet, with lemonade and oranges, was now ordered. After this, followed other three cases in the persons of William Payne, Charles Evans, and Elizabeth Malony; and these were all the cases of distinct scurvy, which, to the knowledge of Dr Hutchison, existed in the Penitentiary up to the 8th of March, when he addressed his first letter on the subject of scurvy among the penitents to the committee.* All of these five cases recovered except the girl, Mary Ann Davison, who could not be expected to live, and who died on the 17th February.

"When I officially announced the existence of scurvy," says Dr Hutchison, "I was unwilling to name the disease to the surgeon, fearing that the alarm would be soon spread over the prison, and perhaps the neighbourhood. It must be evident that my reserve arose from respect to the committee, as I was not responsible for the diet; and had I made it public then, I might perhaps have saved myself from the imputation of inattention, now attempted to be cast upon me, from my not having regularly announced the disease. On the 2d February, I ordered lemon juice for those cases, in addition to the improved diet on which I placed them; but it was not until the 4th of that month that it could be procured from Apothecaries Hall. I then ordered fresh lemons; and on Mr Pratt asking me if he should send for a dozen, my reply was, "send for a couple of baskets." This disease is well known to such as have experience in it, to appear among a ship's company, for instance, affecting only one, two, or half a dozen cases, while the others will remain perfectly free from attack; and hence, I had hoped such might have been the case in the Penitentiary, as it was an entirely new disease in that institution; *and I deny that the disease could be traced farther back in any*

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* This letter does not appear in the evidence; but in a note or a letter of February 14th, where it is referred to, it is stated that perhaps the letter of the 14th is meant, as no letter of the 8th could be found. It is afterwards stated in another note, that this letter was found by the committee, and the assertion is therefore correct.

one individual, than the period I have fixed; it was immediately after the severe cold weather it began to show itself. In Cold Bath Fields prison, and in Norwich Castle, only five cases occurred in each." *Minutes of Evidence, p.* 36.

Dr Hutchison further states that, for the reason now assigned of the manner in which the disease appears, and proceeds among a body of men, he did not examine the legs of the other prisoners in the Penitentiary,—does not suppose that Mr Pratt the surgeon is well acquainted with cases of scurvy, and did not state to him that scurvy was in the establishment till after he had written his letter of the 8th to the committee, nor did he mention it to Mr Anthony White, the consulting surgeon, till he saw it personally, because he was not in the habit of visiting regularly the prison or infirmaries. In letters of the 14th and 24th February, Dr Hutchison informed the Committee of Management, that "slight cases of scurvy were among the prisoners," and that " one well marked case, and two others of less distinct character, had appeared within the last four days;" and in a letter of the 28th, he had said that "he had visited the Penitentiary daily, and found four more cases of scurvy, two of which were attended by flux." When interrogated whether on the 28th February, he had examined the legs and the persons of the prisoners in the Penitentiary, he states that he had not,—but that it was his intention, according to his habit of monthly inspection, to have done so the following day, when he found by resolution of the 28th, that Drs Roget and Latham were called in to have the whole charge of the sick. Admits that in the examination on the 3d March, they found nearly one-half affected; that these persons were in the prison, not in the infirmary, and consequently not under his immediate care; states that those in the prison are under the immediate care of the surgeon (Mr Pratt) between the monthly inspections of the medical superintendent; but that he (the medical superintendent) was always at his duty, and ready to visit any part of the prison to which the committee might call his attention. At a further examination on the 27th May, Dr Hutchison positively states that the dysenteric affection mentioned by Dr Roget in his evidence, as a symptom of scurvy, actually appeared among the convicts after he had announced the disease to the committee in his letter of the 8th February; presumes that the number of 448 cases, stated by one of the physicians in their Report, referred to every individual who showed the smallest spot upon the leg; for these persons were not in the infirmary, or under medical treatment, but in the prison; believes that the infirmary did not contain at this time even more than 110 or

120 cases of any species of disease under treatment, and the other 328 were in the general prison. Has seen a good deal of scurvy in North and South America, in Newfoundland, in the Baltic, in the West Indies, and on the coasts of England and Ireland; has not seen it at land unless in the Penitentiary and in Naval Hospitals, where it appeared in persons confined to bed, without appetite, and very much emaciated. Dr H. farther states, that he concealed the existence of disease in the prison from the neighbourhood, because he knew it was not the wish of the committee that any noise should be made concerning the situation of the convicts at any time, as the people of Lower Westminster were ever ready to find fault with the Institution, and had he made it known at once to the surgeon, it would have been speedily spread round the neighbourhood. He has no reason to believe that the marks appeared on the legs of the prisoners till about the period when he noticed the first case; thinks that a prisoner would have instantly mentioned it either to Mr Pratt or the superintendent himself, if he had noticed any such spots on his legs, and although the appearance of these spots might not have led a person unacquainted with the disease to suspect its existence, yet they might have reported them, not knowing what they were; lastly, that Drs Roget and Latham saw the disease a full month after he had announced its existence, and when it had been during that time gaining ground among the prisoners.

At a subsequent examination on the 12th June, Dr H. was interrogated on the number of deaths, the appearances disclosed by dissection and some similar subjects. After the appointment of Drs Roget and Latham to the management of the patients in the Penitentiary, it appears that many circumstances rendered Dr H.'s duties at the Institution unpleasant, in consequence of which he addressed a letter to the committee, which led to proceedings terminating in his dismissal,—and he accordingly ceased to visit the Penitentiary about the 19th April. He acknowledges that he cannot recollect the deaths between January 1st and April 19th, when his attendance expired, but states that there were two only in January;—which appear to have been the cases of John Watson on the 7th, and Robert Ovendale on the 24th, the first with symptoms of inflammation of the kidneys, the second with those of pulmonary consumption. He thinks that not more than 3 or 4 bodies were examined, which was done by himself in the dead-room, with the assistance of Mr White and Mr Pratt, and in the presence of Drs Roget and Latham. Of these Mansfield was one and

Church another, * and there were other two in whom ecchymotic blotches were found in the intestinal mucous membrane. The subsequent part of the examination is so curious, and gives a view so strange of the manner in which the medical business of some institutions is conducted, that we cannot refrain from giving it at large.

"Was there any statement drawn up of the cases of those persons who were opened, or any minute made in a book?—There was not.

"Did you yourself make any note?—I made no note; the transaction is so recent, and so many of us being present, there can be no doubt about obtaining correct information even now, upon that subject; and if it is wished, I shall be happy to join the other gentlemen in drawing up a report.

"Is it not usual on the opening of bodies on occasions so important as that which appears to have occurred at the Penitentiary, for medical gentlemen to make a note?—When surgeons are called upon by the friends of any person to examine a body after death, for the satisfaction of the friends who employ them, we make a statement and give to those friends, but I am not aware of that being done under any other circumstances.

"Not in public institutions?—No; unless the medical gentlemen are making notes for publication.

"Were those bodies opened previous or subsequently to the inquests?—Always subsequently to the inquest.

"The verdict of the jury had been returned?—Yes.

"You opened the bodies of course with a medical view?—Just so, and with the sanction of the committee for so doing.

"Did you make any report to the committee of the symptoms? —The circumstance of those blotches being found in the alimentary canal of two individuals, is mentioned in the Report of the two physicians to the Committee of the Penitentiary, and subsequently laid upon the table of the House of Commons.

"Were the appearances such as justified your previous notion of the nature of the disease, or not?—I mentioned, (and I hope what I am about to say will not be considered as detracting in the smallest degree from the merits of the gentlemen that were called in); I mentioned, I say, to Dr Roget, previous to the examination of either of those two bodies, that I was quite sure, when they came to be examined, the same appearances would be exhibited on the villous coat of the intestine, as existed on the lower extremities of the convicts labouring under scurvy; and after the examination had taken place, I asked Dr Roget, when those appearances were exposed by dissection, whether he recollected what I stated to him the previous day; he said, *perfectly;* and I noticed it, because it was a thing I had seen occur in seamen who had died of that disease.

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* See Table of Deaths.

" Of what disease?—Scorbutic dysentery.

" Brought on by what?—By the causes to which we have already ascribed the disease." p. 131.

We shall probably have occasion to call the attention of our readers to the system of medical economy herein unfolded; but all that we at present shall remark is, that the statement concerning the pathological results which dissection would disclose, is completely at variance with what Dr Roget says in his evidence of May 16th.

" Q. You mean to say that Dr Hutchison did not tell you what the appearances would be after death, which, upon your dissection, you found had taken place in the morbid body of the party?—A. I do not recollect his stating it." p. 25.

P. 66.—Mr Anthony White, surgeon in Westminster, has been consulting surgeon to the Penitentiary since the commencement of the Institution. His attention was first drawn to the disease of the Penitentiary in March last; no notification of the state of health having been sent during the whole of February. The disease was then reported to him by the physicians who had been summoned, to be scurvy; after this time visited in common with them the pentagons and infirmaries, and agrees with them in regarding it decidedly as sea-scurvy. Mr W. has seen instances of this disease more frequently he believes than most civil practitioners, from his habit of visiting patients in the lower parts of Westminster. In the winter of 1813-14, he attended occasionally the United Workhouse of St Margaret and St John, where there occurred upwards of 20 well-marked cases of scurvy much worse than any yet seen in the Penitentiary. This disease, it was remarked, disappeared spontaneously in the spring, when the weather became warm. Mr White ascribed the disease in this instance to the cold damp weather, with occasional frost, which distinguished the winter of 1813-14, and the general deficiency of vegetable diet; does not regard the late winter (1822-23), as likely to produce scorbutic attacks to the same extent as that of 1813-14, but met occasionally with solitary cases of the disease in Westminster, as happens every winter. He ascribes the disease of the Penitentiary to the long continuance of the winter, and the insufficiency of vegetable matter for food; regards the dietary as it now stands, consisting of animal and vegetable matter, ample in quantity and good in quality, but injurious or insufficient for maintaining health, in consequence of its uniformity; has been disposed lately to consider the Milbank disease as infectious, though the diarrhœa presents nothing which could require its being classed with dysentery; and thinks the disease is on the decline, and the patients look better since the warm weather has commenced, and the diet is more plentiful. Lastly, Mr White has seen no proof of damp or bad air in the Penitentiary, and after attentive examination this morning, does not think taking down the walls a measure at all necessary. p. 69.

P. 80.—Sir James Macgrigor, Director-General of the Army Medical Department, &c. visited the Penitentiary in consequence of a letter from Mr Bathurst on the 22d March 1822, found the patients in a very high state of health, but plethoric, especially the females; ascribed this to their diet, considering, at the same time, their confinement and inactivity; made no change in the plan of dietary, but in a letter addressed to the committee on the 27th April 1822, recommended a reduction of 8 ounces of bread from the male and 2 from the female daily allowance;—and as danger was apprehended from the use of knives and forks, suggested that the animal food should be comminuted and given in the form of soup. Visited the Penitentiary again about the 14th February 1823; found the prisoners, especially the females, of less size than before, but not in bad health; was again at the Institution about the end of March, when scurvy was said to prevail among the prisoners, but saw little severe disease;—and though he has no wish to controvert the statement of the gentlemen in attendance, did not see cases which he could correctly characterize as scurvy.

P. 171.—James Johnson, M. D., is a physician; was in the navy about seventeen years, during which he served in the East and West Indies, the Mediterranean, and the North Sea; had occasion to observe many cases of scurvy, and is practically acquainted both with the nature of the disease and the treatment by which it is attacked; visited the Penitentiary yesterday (June 16th) and to-day; saw at least two-thirds of all the sick there; did not direct his attention to any but those who had bowel complaints and scurvy; and reports the scorbutic cases few, mere relics of the disease; but the bowel complaints very numerous. Dr Johnson thinks it difficult to say, whether these bowel complaints are connected with the original disease alleged to be scurvy, because he has seen dysentery or flux, which is a degree of dysentery, with scurvy and without scurvy; he has seen scurvy with and without dysentery, and he has witnessed many more cases of dysentery and scurvy uncombined than combined; at the same time believes that scurvy, in many instances, predisposes to the bowel complaint. He cannot exactly answer, whether the disease is on the decline; was told by the medical men, that the new patients are now less numerous than formerly; and can say, that all are at present mending. Dr Johnson further states, that the hospitals of the Penitentiary contain no case in which the scorbutic *diathesis* is at all prominent; there were only some spots on the skin, and some, but few, sick of the former affection; supposes nine-tenths of those with sore mouths are under the influence of mercury; could ascertain the immediate causes of death only, such as ulcerations of the bowels, by dissection; but regards these as the effects, and not the causes, of the disease; is to see to-night the body of a patient who died yesterday, and of whom there was a dissection this morning, before he got to the Penitentiary; has opened the bodies of many who have died of this complaint (*dysentery*); a great majority have ulcerations of the bowels, which he considers the effects of disease.

P. 191.—William Burnet, M.D., has served in his Majesty's navy about twenty-seven or twenty-eight years, in most parts of the world, except the East Indies, and has seen many cases of scurvy in different climates. Visited the Penitentiary today, and found a considerable number labouring under a complaint in the bowels, but none dangerous; cannot positively say that there are at present many cases of scurvy, though, from the remains of such appearances, he has no reason to doubt the fact. The appearance of the patients in the Penitentiary does not bear any striking resemblance to the phenomena of scurvy witnessed by Dr Burnet in other countries, but does not infer from this that it is not scurvy; examined the mouths and gums of a great many, and the feet in those instances in which the patient stated that he had livid spots on the legs. Dr Burnet further states, that he saw the body of the man who died yesterday; that the intestines were in a state of ulceration; and thinks this ulceration and the effusion to have been the cause of his death. Believes that this is one of the characters of scorbutic dysentery, but cannot speak from personal knowledge, as he has not made any personal examination of this kind after death by scurvy. p. 192.

Mr Archibald Menzies has been a surgeon in his Majesty's navy since the year 1791; has been in all climates, and has twice performed the circumnavigation of the globe. During this time, has seen and treated many cases of scurvy, and is intimately acquainted with the nature of the disease. Yesterday (June 17th) visited the Penitentiary with Sir Gilbert Blane, examined about thirty-seven of the old standards, that had been four, five, or six years in the prison, and upwards of fifty new-comers, altogether about ninety, and found nearly one-fourth more or less affected with symptoms of real scurvy, many however slightly, and convalescent. Admits that the appearance of scurvy was the same as he had seen at sea, but cannot speak of the diarrhœa, unless he had examined the motions and general appearance of the prisoners. Though unaware that ulceration of the bowels has been general in the fatal cases of the Penitentiary, admits that they are very often ulcerated in cases of diarrhœa on ship-board that terminate fatally; that in the cases with which he met in the Naval Hospital under the inspection of Sir G. Blane, during the American war, they found the intestines ulcerated towards the rectum; that those consequent to scurvy generally have much the same appearance,—redness and suffusion of blood under the surface,—which he believes is a distinctive mark of scurvy; is not sure that ulceration exists in common diarrhœas, but has found it so in dysenteries, which terminated fatally. Mr Menzies, when further interrogated, states positively that land-scurvy is *a kind of hepatic affection of the skin*, quite different from sea-scurvy, and that he never knew a fatal instance of it; that the spongy appearance of the gums is characteristic of scurvy; that the gums are often affected in scurvy where there is little soreness; saw two or three cases where they complained much of soreness of the gums,—proceeding in his opinion from scurvy; but believes that in general, and particularly among those in hospital, the soreness of the mouth, which is so much complained of among the convicts, arises from the mercury. The last question is very remarkable.

" Q. Is it not a peculiarity of the scurvy appearing in the Penitentiary, that there should be so much diarrhœa, and so little appearance of scurvy in the gums, and so little appearance of spots on the skin? A. I am not sufficiently acquainted with the diarrhœa of the Penitentiary, to give an opinion upon that question;—one disease may be owing to that; perhaps that disease may produce the other;—it may arise from spare diet, or various causes. Q. Can you form any idea from what this complaint has arisen? A. No, I cannot."

Nodes Dickenson, M. D., has served as a surgeon in the army since 1795, in Europe, but chiefly in the West Indies; has seen cases of scurvy, but has not had occasion to observe or treat many; has seen and treated cases of diarrhœa in great number and variety. Visited the Penitentiary yesterday, and found the cases to consist chiefly of diarrhœa and simple dysentery, or dysentery without fever; observed no case of scurvy, and only very few, where there were very slight marks of scurvy, mere discoloration,—on the skin. Distinguishes dysentery from diarrhœa in the following manner.

" In dysentery there are discharges of mucus secreted from the surface of the intestines, frequently accompanied with discharges of blood, with great pain, a peculiar symptom, denominated by us *tenesmus;* that, however, occurs in diarrhœa, with liquid evacuations, but frequently and most commonly it has those evacuations simply; the evacuations are generally nothing but blood, without any purulent matter, or any of the watery character of diarrhœa. Upon examining the cases yesterday, though they seemed at first to appear more like simple diarrhœa than dysentery, yet upon more accurate examination, the majority of the patients stated they had *tenesmus*, which would authorise me to call it dysentery." p. 199. Did not see the body of the man who died yesterday, but forms his opinion from personal examination of the cases.

Andrew Baird, M. D., served in the navy thirty-eight years, was for sixteen years inspector of naval hospitals and hospital ships, prison ships, marine infirmaries, and his majesty's ships in commission. Knows no scurvy but sea-scurvy; has heard of a scorbutic disease on shore, but it bears no resemblance to sea-scurvy, is confined to the cuticle, and does not impair the constitution; he should look at the putrid state of the gums as the second stage of the disease, and is quite unaware of the discolouring of the legs extending to any length in scurvy, without soreness of the gums.

Dr Baird was prevented from visiting the Penitentiary on the 17th June, as the committee had expected, in consequence of learning from Mr Capper, at the Secretary of State's office, that an order was issued against the visits of strangers. On the following day, however, by virtue of an order from the committee, he was admitted, and found the following state of the patients. He could not trace any remains of scurvy, or any thing that had any affinity to that disease, unless in one or two cases, in which the shade of disease was so slight, that he should not have recognised it, had he not learnt that the disease had prevailed in the Institution. The disease most prevalent was diarrhœa and dysentery, many cases of which were either very slight or convalescent; many however were under the influence of mercury, and in a state of salivation.

" I was a good deal astonished to see some of them, who were stated to me to have been under the influence of mercury for seven or eight weeks, exhibiting no appearance of their mouths being affected, although taking it to a very considerable degree; not only taking it, as stated, every hour internally, but rubbing it in at night; still there was no appearance of the mouth being affected; and some of those, not a few, exhibiting very much the appearance of health, no wasting of their muscles, and their general health appearing to be good. I could not well reconcile to myself, that patients labouring under such a disease, so as to make their evacuations amount frequently to the number of eight or ten motions in the course of twenty-four hours, that this disease should have gone on for five or six weeks, and the people exhibiting no appearance of impaired health; from which I was induced to infer, that many of those cases had found their way to the infirmary, without having any serious disease upon them. I was led to suspect this, knowing that they would have better quarters there, better food, and not have any labour to perform. On making this observation, however, to an honourable member of the committee, he assured me that the prisoners had not any predilection for the infirmary; he had not any reason to suppose that their cases were feigned. I inquired what evidence there was of this frequency of motions, whether they were examined each time or not; conceiving that some of their evacuations might have been preserved in the infirmary, I desired to see some of them. I found the practice was for them to go to the common necessary; therefore, there was none reserved for my inspection. The surgeon informed me, that he had inspected their evacuations, and was satisfied in most cases they had the disease. I, of course, am bound to believe that statement; but I can scarcely know how a man could attend to so many patients, and examine their evacuations. Had their evacuations been made in the common way of hospitals, having a bed-stool in the room, the thing could have been ascertained in a very tangible shape, and the nurses would be able to speak to it; but not having any such evidence, I still remain in doubt as to the generality of the cases being real, inasmuch as the general health not being, as you would naturally expect it to be, impaired, after such a confinement, and labouring under such a disease. I must still have my doubts of the reality of many of the cases; and I should suppose, that not only does it become necessary to ascertain that the disease does exist by such inquiry, but also, in a medical point of view, it becomes necessary for a medical man, for his own satisfaction, to enable him to prescribe for the patient, in order to see what change is produced by the course he is pursuing." pp. 237, 238.

Notwithstanding these doubts of Dr Baird, the disease was not fictitious or simulated. Dr Augustus Granville visited the Penitentiary on the 19th June in company with Dr Baird, and after examining many of the patients with great attention, gives the following account of their condition.

"With some few exceptions, all the prisoners I saw or examined have been labouring under various modifications of a bowel complaint, for two, and, in some few cases, three months, with more or less intensity in the attack, so that in some it appeared as common diarrhœa attended with pain in the belly, &c., and in others, as dysentery with fever, and in some few cases, with the appearance of blood in the evacuation. Some were convalescent from the first and only attack they had experienced; some were recovering from a first and a second relapse they had had after recovery; others were ill in bed with fever and some symptoms of active disease then going on; some who were ill and complaining had had only one or two evacuations a day, while others had had eight, nine, ten, eleven, and even twelve.

"What was the number of the prisoners you found under medical treatment on the day you visited it?—Up to yesterday, I understood, from inquiry, that there were 450, or thereabouts, on the sick list, in the total population of a little more than 850 prisoners; so that nearly one half might be said to be under medical attendance; but it is possible that number may have even increased to-day; for I found, upon inquiry of Mr Pratt, out of the number of admissions taken at random for any fifteen days, that the average number admitted daily was about twelve. I do not mean to say it is so now." p. 242.

With this strong evidence of the existence of an intestinal disease, we regret we cannot contrast the evidence of the non-existence of the scorbutic malady. Dr Granville, believing the existence of scurvy to be generally admitted, did not inquire into the proofs of its presence.

"The previous existence of scurvy among the prisoners in the Penitentiary, together with the presence of both spongy gums and spots on the legs in some of them, having been admitted, as I thought, on all hands, both in evidence and documents before the Committee, corroborated by the testimony of a person whom I cannot but consider as the best witness examined here (I mean Sir Gilbert Blane), I thought it was scarcely necessary for me to direct my attention to that point, so much as to the disease now prevailing, and the treatment of it." pp. 242, 243.

With this extract we close our evidence on the nature of the disease: and we believe our readers will admit that it is sufficiently contradictory, and contains statements from which, considering the character of the witnesses, it is very difficult to form a decided opinion. The questions are often improperly or incorrectly proposed, and the answers deficient, in some instances, in precision; and the result of the whole is, that little or no accurate information on the phenomena of the disease, and on the exact degree of these phenomena is communicated. We have, however, bestowed considerable care in selecting those points only and principally, the knowledge of which would elucidate the question of the nature of the disease or diseases in the Penitentiary; and, as we deem it of the utmost consequence to form our conclusions from matters of fact as positive as the case admits, we have, in the preceding abstract, been more solicitous of scrupulous fidelity in reporting and abridging, than of elegance in communication. We fear, indeed, that the Committee of Inquiry have gone too much on the principle of the disease being positively and uniformly scurvy, and have presumed that to be demonstrated as matter of fact, which still required investigation and scrutiny.

The first point which appears to us to be doubtful, is, whether the disease was actually scurvy, and if it was, to what extent it prevailed among the penitents. The second point which requires to be examined is, whether there were not other diseases, as diarrhœa or dysentery, whatever denomination they receive, and whether these were necessarily connected, as alleged by Drs Roget and Latham, with the scorbutic symptoms.

It will be found, in glancing at the evidence which we have now submitted to our readers, that it is by no means a unanimous opinion among the medical witnesses, that the disease of the penitents was scurvy; and that several, if they did not actually deny its existence, declined saying positively that it was. Thus, while Drs Roget and Latham and Dr Hutchison assert the disease to be scurvy, and Sir Gilbert Blane admits that it is distinct scurvy, but in a mild form, Sir James Macgrigor will not say that he saw cases to which he would give this appellation. Dr Johnson finds almost no cases with the scorbutic diathesis. Dr Burnet positively states that the cases of the Penitentiary bear no resemblance to the cases of scurvy witnessed by him in other countries. Dr Dickenson either observed no case of scurvy, or found marks so slight that they scarcely deserved the name. Dr Baird could trace no evidence of the disease on the 19th June, and admits its existence on the testimony of Dr Hutchison only; and Dr Granville, at the same time, though he found many patients labouring under diarrhœal or dysenteric complaints, saw no cases of scurvy or scorbutic symptoms. This ambiguity and contradiction reduces the question to a very embarrassing alternative. It is admitted by all the witnesses that Dr Hutchison is intimately acquainted with the phenomena of scurvy, and he is the first to announce its existence. It is impossible to doubt that Sir Gilbert Blane is familiar with its appearances; yet, as we shall immediately see, when the evidence on which his opinion is founded is examined, it possesses much less force than is requisite to produce conviction.

It is a remarkable circumstance in the present inquiry, that there is no proof that, in any of the alleged cases of scurvy, the mouth was ever affected. The first and almost the only symptoms throughout were ecchymotic spots or blotches of various size; appearances, it may be remarked, which may be referred to one or other of the forms of *purpura* or land-scurvy. Not only is the sponginess of the gums, so uniformly mentioned by all practical observers, never noticed in the evidence either of Dr Hutchison or of Drs Roget and Latham, but we have positive proof that it did not exist. We find, for example, in the ostentatious reports of Mr Pratt, of 27th February, the following positive remark.

> " There has been a great number of slight attacks of diarrhœa in the pentagons, but which are doing well; this has been both in the males and females; and there is, I apprehend, the scurvy among them, which has made its appearance on their legs, having discovered spots, although the grand mark distinguishing that disease, the wasting and spongy appearance of the gums, had not taken place."

Another very extraordinary fact to the same purpose, and establishing in our minds the same conclusion, is brought out in evidence in the following manner.

Our readers cannot yet have forgotten the evidence of Mr Menzies and of Dr Baird, with regard to the state of the gums, proceeding respectively from scurvy or from the administration of mercury. Sir Gilbert Blane, when examined on the 27th May regarding the proportion of persons whom he found labouring under scurvy at his visit on Saturday the 24th, states, that he found sea-scurvy in a slight form, but without ambiguity, and perfectly marked, affecting a large proportion of the penitents—a good third;—and when further examined on the state of their health, we find the following remarkable answer.

> " Their general health was good; their only complaint was a distant characteristic of sea-scurvy, namely a peculiarly rough appearance of the skin, with small brown spots like flea-bites, and some degree of swelling and hardness of the muscles; a hard feeling and some degree of inflammation about the ankles; and about one-half of them had sore gums." (*Minutes*, p. 60.)

And this statement is substantially confirmed in general terms the following day, (28th May), when he admits that the 30 persons from whose appearance he formed his opinion, were under no medical treatment, (63.) Yet a few days after, viz. 2d June, we find Dr Latham come forward and make the following statement to the Committee.

> " The other day it was stated by Sir Gilbert Blane, that two-thirds of the prisoners laboured under a considerable degree of scurvy; he took his test from the state of the men's gums, without looking at their legs; and it is quite clear, that in some of those in whom he considered scurvy to exist, by that test, their gums were affected by mercury. Q. He was not aware of the treatment they were under? A. I believe he overlooked it; we suggested it in a case or two." (p. 103.)

Now, if we give this statement the weight, which its author undoubtedly claims, and which we cannot in common justice withhold, what is the result? In the first place, that the persons of the inmates of the Penitentiary exhibit not a single phenomenon to which the character of a pathognomonic symptom can be applied. "Their general health was good; and the only distant characteristic of sea-scurvy which they presented, was a rough appearance of the skin, with small brown spots like flea-bites, and some degree of swelling and hardness about the ankles." The second result of the admission made by Dr Latham, is, that we are compelled to ask what were the circumstance or circumstances on which Sir G. Blane could found his opinion of the presence of scurvy among the penitents? Not certainly on the brown spots alone, or on the swelling and hardness of the ankles; much less could it be said that scurvy was present where the "general health was good;" for this in truth is a contradiction in terms. Where, it may be asked, is the doughy or pasty state of the soft parts, which distinguishes all cases of scurvy from the earliest periods of the disease; where are the hemorrhages either from the skin or the mucous surfaces, without which the malady can scarcely be said to exist; and how does it happen that neither the gums nor teeth were affected from the earliest notice in Mr Pratt's Journal, down to the latest reports by Sir Gilbert Blane, Dr Burnet, Mr Menzies, and Dr Baird? There was assuredly, under such circumstances, a strong reason for the question of the Committee to Mr Menzies; "If it was not a peculiarity of the scurvy in the Penitentiary, that there should be so much diarrhœa, and so little appearance of scurvy in the gums, or spots on the skin" (p. 198.); and an equally strong reason for the ambiguous and evasive answer of Mr Menzies. So great a peculiarity, we should say, that it ought to have made all concerned pause, and examine the cases both in extent and degree with much greater attention, ere they declared that scurvy was the malady.

Is it therefore to be inferred, it will be asked, that scurvy did not appear among the convicts of the Penitentiary during the early months of 1823? Is it possible that medical witnesses of acknowledged experience were deceived as to the nature of the complaints with which the penitents were afflicted? Were the first cases of illness which Dr Hutchison named scurvy any thing else, and is it to be supposed that a physician of his practical knowledge could be mistaken? We will not attempt to disguise the extreme difficulty to which these questions give rise, nor pretend to deny, that these cases are liable to the same objections which we have shown apply with such force to the general prevalence of the disorder throughout the Institution. But if we admit these cases to be genuine scurvy, and in this we plead the same reason which Dr Baird has done, it by no means follows that this disorder affected the penitents in the prodigious extent to which it is represented subsequently to have proceeded. Those cases, indeed, in which genuine scor-

PRESENT DIETARY:—Since July 1822.

IORNING - Three quarters of a pound of bread and one pint of gruel, for the males; and nine ounces of bread and three quarters of a pint of gruel, for the females.

R - - - Three quarters of a pound of Bread and one pint of Soup, for the Males; and nine ounces of Bread and three quarters of a pint of Soup, for the Females.

VENING - One pint of Soup for the Males, and three quarters of a pint for the Females. *

oup to be made with Ox heads, in lieu of other meat, in the
on of one Ox head for about 100 Male Prisoners, and the
about 120 Female Prisoners; and to be thickened with Ve-
and Peas, or Barley, alternately, either weekly or daily, as
ound most convenient.

Committee to substitute, at their discretion, Potatoes for
t the rate of 1 lib. of Potatoes for ½ lib. of Bread.

nly Liquor allowed to Prisoners in health (except Broth or
hall be water.

ers confined to Bread and Water Diet for punishment, shall
ed such quantity of Bread as the Surgeon may think neces-

ers employed in Works of extraordinary Labour, or under
ances which may render it necessary, may be allowed an
to the quantity of their Provisions, by the direction of the
ee.

men, and those employed in the Kitchen and Bakehouse,
allowed ½ lib. of Bread extra, daily.

e Prisoners employed in the Laundry shall be allowed 1½ lib.
l, daily, with a double allowance of Soup at Dinner and Sup-
he days in which they are employed in Washing; viz. Mon-
esdays and Wednesdays.

swomen, and those employed in the Kitchen, shall be allowed
f Bread, daily.

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y Gruel has been substituted for Soup of an Evening for the Females.

FORMER DIETARY:—Previous to July 1822.

DAILY,—1½ lb. of Bread, made of such Meal as the Committee may from time to time direct, for every Male Prisoner above the age of 18 years; and 1 lb. for every other Prisoner. But Boys under 18 may have the full allowance of Bread, if the Chaplain and Governor shall certify, at the commencement of the week, that their conduct has been meritorious during the week preceding.

For Breakfast		1 pint of hot Gruel or Porridge.
For Dinner	Sundays, Tuesdays, Thursdays, Saturdays,	6 ounces of Clods, Stickings, or other coarse pieces of Beef (without Bone, and after boiling), with half a pint of the Broth made therefrom. 1 lb. of boiled Potatoes.
	Mondays Wednesdays Fridays	1 quart of Broth for the Males, and 1 pint for the Females, thickened with Scotch Barley, Rice, Potatoes or Peas, with the addition of Cabbages, Turnips, and other cheap Vegetables. 1 lb. of boiled Potatoes.
For Supper		1 pint of hot Gruel or Porridge.

N. B.—Prisoners may reserve such part of the Provisions previously delivered out, as they please, for their Supper.

Salt and Pepper, as the Committee shall from time to time direct.

The only Liquor allowed to Prisoners in health (except Broth, Gruel, or Porridge) shall be water.

Prisoners confined to Bread and Water Diet for punishment, shall be allowed such quantity of Bread as the Surgeon may think necessary.

Prisoners employed in Works of extraordinary Labour, or under circumstances which may render it necessary, may be allowed an addition to the quantity of their Provisions, by the direction of the Committee.

MEMORANDUM:

Female Prisoners employed in the Washhouse shall be allowed an addition of ½ lb. of Bread and an allowance of Meat on Washing Days, until the further orders of the Committee.

By Order of the Committee,

A. MACDONALD.

PRESENT DIETARY:—Since July 1822.

In the Morning	Three quarters of a pound of bread and one pint of gruel, for the males; and nine ounces of bread and three quarters of a pint of gruel, for the females.
At Noon	Three quarters of a pound of Bread and one pint of Soup, for the Males; and nine ounces of Bread and three quarters of a pint of Soup, for the Females.
In the Evening	One pint of Soup for the Males, and three quarters of a pint for the Females. *

The Soup to be made with Ox heads, in lieu of other meat, in the proportion of one Ox head for about 100 Male Prisoners, and the same for about 120 Female Prisoners; and to be thickened with Vegetables and Peas, or Barley, alternately, either weekly or daily, as may be found most convenient.

The Committee to substitute, at their discretion, Potatoes for Bread, at the rate of 1 lib. of Potatoes for ½ lib. of Bread.

The only Liquor allowed to Prisoners in health (except Broth or Gruel) shall be water.

Prisoners confined to Bread and Water Diet for punishment, shall be allowed such quantity of Bread as the Surgeon may think necessary.

Prisoners employed in Works of extraordinary Labour, or under circumstances which may render it necessary, may be allowed an addition to the quantity of their Provisions, by the direction of the Committee.

Wardsmen, and those employed in the Kitchen and Bakehouse, shall be allowed ½ lib. of Bread extra, daily.

Female Prisoners employed in the Laundry shall be allowed 1½ lib. of Bread, daily, with a double allowance of Soup at Dinner and Supper, on the days in which they are employed in Washing; viz. Mondays, Tuesdays and Wednesdays.

Wardswomen, and those employed in the Kitchen, shall be allowed 1½ lib. of Bread, daily.

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* Lately Gruel has been substituted for Soup of an Evening for the Females.

TABLE of the Deaths among the Convicts of the Grand Penitentiary at Milbank, Parish of St John the Evangelist, Westminster, from January 1st, 1823, to June 19th.

Penitent's Name.	Age at admission.	Date of admission to Penitentiary.	Date of admission to Infirmary.	Date of Death.	Age at Death.	Symptoms.	Alleged Disease and Cause of Death.	Remarks.
1. John Watson	30	1. June, 1822.	7. Dec. 1822.	7. Jan. 1823.	30	Pain of loins and region of heart.	Inflammation of kidneys and heart.	
2. Robert Ovendale	22	7. June, 1820.	2. Oct. 1822.	24. Jan. 1823.	24	Pains of chest, cough, purulent expectoration.	Inflammation and suppuration of lungs. Consumption.	
3. Peter Prest	29	25. Feb. 1822.	2. Feb. 1823.	14. Feb. 1823.	30	Vomiting and purging—14 stool daily.	Diarrhœa.	Inflammation of mucous membrane of intestines.
4. Mary Ann Davison	15	3. June, 1820.	30. Dec. 1822.	17. Feb. 1823.	18	Languor, irritation of heart and lungs, expectoration.	Consumption.	Catarrhal consumption.
5. Mary Esp	21	5. June, 1821.	9. Feb. 1823.	19. Feb. 1823.		Cough, diarrhœa, scrofulous sores of neck, with hemorrhage.	Supposed abscess of chest.	Unknown.
6. William Condwell		7. June, 1820.	5. Jan. 1823.	23. Feb. 1823.	30	Diarrhœa, dropsy of chest.	Unknown. *See the verdict on this man, p. 153.*	
7. Humphrey Adams		1. June, 1821.	Feb. 1823.	24. Feb. 1823.	34	Diarrhœa—12 or 14 stools daily.	Diarrhœa.	Inflammation of mucous membrane of intestines.
8. Margaret Patterson	19	18. April, 1820.	12. Jan. 1823.	28. Feb. 1823.	22	Diarrhœa—10 or 12 stools daily.	Diarrhœa.	*Idem.*
9. Henry Jew	46	13. March, 1822.	11. Feb. 1823.	8. March, 1823	47	Diarrhœa.	Diarrhœa.	*Idem.*
10. Robert Dyer		9. Nov. 1820.	15. Feb. 1823.	8. March, 1823.	31	Sickness, pain of chest, spasms of stomach.	Supposed effusion of blood in head and chest.	Unknown.
11. Thomas Read		11. Feb. 1817.	6. Feb. 1823.	9. March, 1823.	20	Rheumatic pain of loins, dropsy.	Supposed effusion of water in the chest. *See verdict, p. 155.*	
12. James Jones		17. Aug. 1821.	18. Feb. 1823.	10. March, 1823.	29	Violent purging—15 stools daily.		Inflammation of mucous membrane of intestines.
13. Ann Collins	45	2. Nov. 1820.	7. Feb. 1823.	13 March, 1823.	48	Pains of chest, and inflammation of lungs.	Inflammation of lungs.	
14. Ann James	18	1. Sept. 1819.	10. Feb. 1823.	16. March, 1823.	22	Nausea, headach, pain of stomach, jaundice.	Supposed to be jaundice. *See verdict.*	Fever, or liver disease?
15. Thos. Peter Mansfield	16	18. Feb. 1817.	3. March, 1823.	22. March, 1823.	22	Violent pains of the head, terminating in apoplexy.	Apoplexy.	
16. Harriet Church	23	23. June, 1820.	1. Feb. 1823.	23. March, 1823	26	Headach, loss of sight, incoherent speech.	Supposed effusion of blood in the head or heart. *	
17. John Guest	22	7. Dec. 1821.	21. Feb. 1823.	25. March, 1823.	24	Scrofula, scrofulous swelling of knee, neck and legs.	Suddenly. Supposed rupture of blood-vessels of lungs. *See verdict.*	No examination.
18. Frances Alcock		24. Dec. 1821.	9. March, 1823.	28. March, 1823.		Diarrhœa, repeated attacks after inflammation of lungs.	Diarrhœa.	
19. Wilson Bragg	32	6. Jan. 1821.	28. Feb. 1823.	31. March, 1823.	34	Diarrhœa.	Diarrhœa.	Inflammation of intestinal mucous membrane.
20. Louisa Counforth		26. Nov. 1821.	25. Jan. 1823.	14. April, 1823.	26	Violent spasms in stomach and bowels.	Spasms of stomach and bowels.	
21. Catherine Mooney		25. March, 1819.	1. May, 1822.	14. April, 1823.	15	Scrofulous sore of neck.	Alleged disease of lungs.	
22. Ann Martin	21	29. Oct. 1819.	19. Jan. 1823.	20. April, 1823.	25	Deformed spine, palsy of legs, and sphincters—bedridden excoriation.	Erysipelas, terminating in mortification.	Diseased spine, and its consequences.
23. Jane M'Vickers	13	8. May, 1820.	9. Dec. 1822.	19. April, 1823.	16	Cough and chest complaints.	Scrofula affecting the lungs.	Inflammation of lungs terminating in consumption.
24. Thomas Brown	20	9. May, 1822.	6. Jan. 1823.	25. April, 1823.	21	Severe pain of head, occasional hurried and laborious breathing, irregular circulation, nervous irritation.	Symptoms as enumerated.	Disease of brain. No dissection.
25. Ann Adams	25	8. July, 1819.	15. March, 1823.	27. April, 1823.	29	Rheumatism, disorder of stomach and bowels.	Debility.	No dissection.
26. Saray Farley		30. Oct. 1820.	15. March, 1823.	3. May, 1823.	26	Violent pain of stomach, supposed to be spasms, accompanied with fits.	Fits.	No dissection.
27. Ann Williams		3. June, 1822.	3. April, 1823.	5. May, 1823.	32	Pain of region of the heart.	Supposed inflammation about the heart.	No dissection.
28. Mary Venables		9. July, 1819.	1. April, 1823.	17. May, 1823.	20	Violent pains of head, drowsiness.	Apoplexy from supposed effusion of blood in the brain.	No dissection.
29. Elizabeth Lowton	16	11. Oct. 1819.	15. Feb. 1823.	25. May, 1823.	20	Pain of chest, cough, and diarrhœa, afterwards purulent expectoration.	Consumption.	
30. William Dongworth	19	23 Nov. 1820.	17 Feb. 1823.	9. June, 1823.	22	Diarrhœa—10, 12, 16, or 18 stools daily.	Diarrhœa.	Inflammation of mucous membrane of intestines.

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* In the Report of the dissection by Dr Hutchison—effusion of water in the brain.

TABLE of the Deaths inster, from January 1st, 1823, to June 19th.

Penitent's Name.	A and Cause of ath.	Remarks.
1. John Watson	idneys and heart.	
2. Robert Ovendale	suppuration of ption.	
3. Peter Prest		Inflammation of mucous membrane of intestines.
4. Mary Ann Davison		Catarrhal consumption.
5. Mary Esp	f chest. *e verdict on this*	Unknown.
6. William Condwell		Inflammation of mucous membrane of intestines.
7. Humphrey Adams		*Idem.*
8. Margaret Patterson		*Idem.*
9. Henry Jew	of blood in head	Unknown.
10. Robert Dyer	of water in the *ct, p.* 155.	
11. Thomas Read		
12. James Jones	ungs.	Inflammation of mucous membrane of intestines.
13. Ann Collins	undice. *See ver-*	Fever, or liver disease?
14. Ann James		
15. Thos. Peter Mansfield	of blood in the	
16. Harriet Church		
17. John Guest	sed rupture of ungs. *See verdict.*	No examination.
18. Frances Alcock		
19. Wilson Bragg	and bowels.	Inflammation of intestinal mucous membrane.
20. Louisa Comforth		
21. Catherine Mooney	lungs.	
22. Ann Martin	ating in mortifi-	Diseased spine, and its consequences.
23. Jane M'Vickers	the lungs.	Inflammation of lungs terminating in consumption.
24. Thomas Brown	nerated.	Disease of brain. No dissection.
25. Ann Adams		No dissection.
26. Saray Farley		No dissection.
27. Ann Williams	ation about the	No dissection.
28. Mary Venables	posed effusion of in.	No dissection.
29. Elizabeth Lowton		
30. William Dongworth		Inflammation of mucous membrane of intestines.

butic symptoms took place, appear to have been not only very mild, but limited to a very small number; and we cannot by any sophistry persuade our minds to agree with Drs Latham and Roget in regarding cases of slight spots, which are exceedingly common among the lower orders, as instances of that disease, which has desolated ships crews, and paralysed the exertions of our navy.

The evidence of the prevalence of scurvy in the Penitentiary has not, so far as we hitherto have gone, been either clear or convincing. If we penetrate a little farther, and divest the inquiry at the same time of all the circumstances which tend to perplex it, we find that the little evidence which it hitherto possessed, is not only weakened, but is first neutralised, and then converted, in a great measure, into a positive contradiction. It might be presumed, that whatever was the nature of the disease which gave rise to so much alarm, it should show its proper character in the proportion of deaths which took place in the Institution, during the period that it was stated to have exercised its ravages. It was farther to be presumed, that, if, during the period to which we allude, it could be shown that the cases of severe and fatal disease which were most prevalent in the Institution were referable, not to scurvy but to other maladies; the assertion that the disorder of the Penitentiary was scorbutic, would lose all its force, and it would follow, that the disease and mortality proceeded from other causes. Impressed with these principles, the justice of which we conceive to be undeniable, we examined with the utmost care the reports of the deaths which had taken place in the Penitentiary between the 1st January and the 16th June, 1823, the period at which the investigation was going on; and we have, for various reasons, digested them in a tabular form, from which the principal results will be easily understood. The first or general table consists of eight columns in which the following elements are respectively arranged. 1st, The penitent's name; 2d, Age at admission if it can be ascertained; 3d, Date of admission to the Penitentiary; 4th, Date of commencement of complaints and admission to the infirmary; 5th, Date of death; 6th, Age at time of death if it can be ascertained; 7th, Prominent symptoms as stated in the report of the surgeon; 8th, Alleged disease as stated in the report of the surgeon or of the coroner; and we add, in a 9th, such hints on the nature of the disease as we think requisite or borne out by the evidence. The second table is compiled from this one, and contains the numbers of deaths, according to the diseases of which they died; showing thus, at a single glance, the relative prevalence and mortality of different diseases in the Penitentiary.

DEATHS in which the prominent symptoms were in the head.	DEATHS in which the prominent symptoms were in the chest.	DEATHS in which the prominent symptoms were in the bowels.	DEATHS in which the prominent symptoms were cramp or spasms of stomach.	DEATHS in which the prominent symptoms were affection of heart.	DEATHS in which the prominent symptoms were scrofulous sores.	DEATHS in which the prominent symptoms were dropsical effusion.
Thos. P. Mansfield.	Robert Ovendale.	Peter Prest.	Robert Dyer.	Ann Williams.	Mary Esp.	Thomas Read.
Harriet Church.	Mary A. Davison.	Humphrey Adams.	Ann James.	John Watson.	John Guest.	
Thomas Brown.	William Condwell.	Margt. Paterson.	Louisa Comforth.		Catharine Mooney.	
Mary Venables.	Ann Collins.	Henry Jew.	Ann Adams.		Ann Martin.	
	Jane M'Vickers.	James Jones.	Sarah Farley.			
	Elizabeth Lowton.	Frances Alcock.				
		Wilson Bragg.				
		William Dongworth.				

From this table, therefore, we learn with considerable accuracy the nature of the diseases which proved most fatal; and we are, at the same time, enabled to judge of the evidence for believing in the prevalence of scurvy or scorbutic dysentery. Of the thirty persons who died at the Penitentiary between the 1st January and the 16th June, we find that the diseases were not at all the same, nor even was the proportion of intestinal or dysenteric disease very preponderating. There are four deaths of persons who laboured under complaints indicating disease of the head, and in whom not a single symptom capable of being mistaken for scurvy appears to have occurred. Not fewer than six, at the lowest calculation, died with symptoms indicating disease of the heart or lungs or some form of consumption, and in whom, if the bowels were disordered, they were so in a secondary manner only; and if we add to these the cases reported by the surgeon as affections of the heart, they will amount altogether to nine, the greater number of the thirty. Five died with symptoms of alleged cramp or spasm or other disorder of the stomach. One died with symptoms of dropsy, and was supposed to have dropsical effusion of the pleura; three had scrofulous sores externally, and as it is conjectured, internal disease of the same kind; and Ann Adams, who is reported to have died of debility from repeated attacks of rheumatism, with disorder of stomach and bowels, appears to have had no symptom which could possibly be ascribed to scurvy. Lastly, the case of Ann Martin, about which so much nonsense is uttered by the surgeon, by the coroner, by her relations, and by all concerned, was doubtless disease of the spine, or one or more of the vertebrae, thus causing injury to the vertebral chord, palsy of the lower extremities and the sphincters, and finally, bed-ridden excoriation, a species of erythema, and not *erysipelas* inflammation, as Mr Pratt has learnedly termed it. That the death of this unfortunate girl was accelerated, or perhaps was directly caused by the final erythematous and gangrenous inflammation, is very possible; but that she became unwell and disordered in her body, by the short allowance and the quality of the provisions given her, as is gravely stated in the verdict, is the most ridiculous and absurd assertion that ever issued from a jury.

Among these thirty deaths, however, there are eight cases in which the intestinal canal was decidedly and eminently the seat of morbid action; but if these cases be attentively examined, so far at least as this is practicable, in the absence of the evidence of dissection, which appears to have been attemped in two only, it will become somewhat doubtful, whether they are to be regarded as genuine examples of scurvy, or are not to be referred to a very different head.

We have, in the *first* place, to remark, that in general their duration is greatly too short to allow them to be referred to this head; for, in two cases only,—Patterson and Dongworth,—did the morbid action last so long as it generally does in scurvy or scorbutic dysentery. Patterson's disease appears to have lasted from the 12th January to the 28th February, forty-seven days, and that of Dongworth lasted from 17th February to 9th June, a period of 112 days, or nearly four months. Of the other six cases, one lasted 25 days, two 20, one 19, one 12, and one only 3,—all of them periods too short to admit of their being regarded as examples of scurvy. The accounts of these cases, as given in the reports of Mr Pratt, are exceedingly meagre, vague, desultory and unsatisfactory; and none of them, two excepted, bear proof of having been ascertained by dissection, so that it is a matter of considerable difficulty to speak of them with precision. But, taking into consideration at once the collateral circumstances, and the direct fact of their being in general cases in which from 10 to 16 fluid stools were voided daily, while, in the few fatal cases which were examined, ulceration was found to have taken place in the intestinal mucous membrane, *(Evidence of Dr Baird, Dr Johnson, and Mr Menzies, last Report of Drs Roget and Latham)*, it is difficult to regard them as any thing else than examples of inflammation of the mucous membrane of the intestines, whatever be the exciting or *material* cause of that inflammation.

In the *second* place, the Report of Drs Latham and Roget presents a view of the connection of scurvy and diarrhœa or dysentery, which has the air of being employed to prove that they are the same disease, or vicarious morbid affections,—appearing, that is to say, alternately for each other. Two cases, it has been said, were dissected, and in these the intestines presented ecchymotic spots or blotches,—a scorbutic state of the intestinal membrane. We are unwilling to say, that these eminent and learned persons have carried their general conclusions farther in this instance than the mere facts which their two dissections disclosed, should have warranted them to do; but suppose it were proved, that in every one of the eight cases the mucous membrane of the intestinal tube was studded with patches of effused blood and ecchymotic spots, it would by no means follow that the malady thus formed was scurvy. These phenomena take place, it is well known, not only in dysentery, but in many cases of diarrhœa, so called, or rather what ought to be termed, in strict language, *enteria*,* inflammation of the mucous coat of the intestinal canal, and of which the fluid stools and tenesmus or dysentery are mere symptoms. We have seen the same morbid changes take place in the intestinal canal at the close of continued fever, and give rise to the same frequency of liquid stools; but we acknowledge, with shame for our want of sagacity, never thought that cases of this kind were to be regarded as genuine scurvy.

In the *third* place, we do not go so far as Dr Baird seems disposed to do, in doubting the existence of the dysenteric affection (p. 113, 114.) We can easily conceive it to have been the principal, and almost the only disease among the penitents; and though there was ample room for simulation, and every species of collusion, even with this disease, we believe from the general testimony, that it may have been at one time, and especially during the cold weather, rather extensive and diffused. At the same time, we cannot comprehend why so little pains were taken to render this quite certain;—why the evacuations were not examined by the medical attendants;—and why there appears in general such a lack of observation, as the ground of medical opinion on the disease. The whole inquiry indeed shows, that little pains have been bestowed in general on the medical management of the convicts; and that the complaints with which they were afflicted were presumed, upon very slight grounds, and very superficial examination, to be what they are alleged. Of many cases of death, in which the symptoms were very complicated and obscure, and in which there is reason to believe they were caused by organic changes in one or more of the internal organs, no dissection appears to have been performed; and the origin and nature of the symptoms appear to have been left in their original ambiguity. In others, again, in which the death was sudden or unexpected, and in which every circumstance concurred to waken the curiosity of the medical observer, instead of any effort being made by dissection to dispel the uncertainty, we find a bold and off-hand assertion, of effusion of blood in the brain or in the chest, or internal hemorrhage, somewhere or other; or even a fit is sometimes had recourse to, as the channel by which the soul is supposed to escape from the body. This method of observing disease, and explaining the event of death, if not novel or uncommon, is at least recommended by its singular facility, and by the small degree in which it requires the medical attendant to exercise either his powers of observation, or his talent of drawing inferences.

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* We use this term to denote inflammation of the mucous surface of the intestines, in preference to *enteritis erythematica.*

II. It was natural to suppose that the committee would be more anxious to ascertain the probable causes of the diseases of the Penitentiary, than their mere nature; and it is easy to discover that this has been one of their leading objects in the examination of the medical witnesses. We find, for example, that the greater part of the questions put to most of these witnesses, relate to the influence of food, the nature and effects of different modes of life, the probable effects of the dietary, which was introduced in July 1822; the influence of cold, of confinement, of inaction, of mental despondency, and other moral causes; in short, of every agent or agents to which the formation of disease has ever been ascribed. It does not appear, however, that much light has been thrown on the subject, and it is quite evident that no positive or unequivocal results have been obtained.

The opinions of Doctors Roget and Latham may be easily gathered from the Report of the 5th April,—which is on the whole substantially confirmed by the answers given in their subsequent appearance before the committee. It is their opinon that the principal cause has been the deficient quantity of nutriment in the diet ordered in July, and its too liquid form; and that the cold weather had exerted an accessory influence; though they admit that it is impossible to say whether a milder winter would have prevented the appearance of the disease. They further add, that greater gaiety, or more cheerful habits than are compatible with long confinement, might have had considerable effect in preventing the appearance of the complaint. In the evidence of Dr Hutchison on the causes and formation of the disease, there is a good deal of tedious and not very interesting inquiry on the dietary of the Penitentiary at different periods, and its influence on the health of the prisoners. It is perhaps as well therefore to lay before our readers the principal facts relative to the diet of the Penitentiary, before we take much notice of this investigation, and more especially as frequent allusion is made to the insufficiency of the prisoners' food during the progress of the inquiry.

We learn from the Report of the Select Committee, that previous to July 1822, the dietary of the Penitentiary had provided for the convicts more plentiful meals than appears to have been consistent either with the system of penal confinement and prison discipline, or was necessary or conducive to health. A glance at this diet-table, which, with the subsequent one we give above at page 119, will at once satisfy the reader that the penitents were abundantly well fed, considering their rank in life, their kind of labour, and the object with which they were committed to the Institution. But it may be remarked, that nothing but actual experience could show whether food of the quality and quantity prescribed, was to be sufficient or injurious to the individuals thus sustained. It appears, accordingly, that in the spring of 1822, the health of the penitents presented changes which both the Managing Committee and the medical superintendent, Dr Hutchison, ascribed to the plethoric effects of too liberal diet. In Dorchester gaol, the same effects, it is said, had proceeded from the same cause, and disappeared on diminishing the allowance of food to the prisoners. This success encouraged the committee to try the same plan; and by the advice of Mr Morton Pitt, a member of the committee, the scale of diet-allowance was reduced, and submitted, with the previous or old diet-table, to the inspection of Sir James M'Grigor, whose opinion that the old dietary was too abundant, and that the one now proposed was sufficiently nutritious, decided the committee. The new or reduced dietary was therefore adopted in the month of July 1822. This innovation, our readers must understand, is regarded as the source of all the evil. It is to this inauspicious change that the whole subsequent train of calamities, with which the Penitentiary has been visited, is ascribed. For though the witnesses in general admit, that this cause did not operate alone, and could not alone generate the diseases which prevailed, they however regard its individual influence as much augmented, in a situation where inaction and the depressing passions were predominant, and at a season distinguished above many that preceded it for its inclemency and long continued cold weather.

As the two first cases of scorbutic spots appeared in the infirmary, and as it is not unusual for scurvy to attack persons confined to bed, while others who are going about resist it, Dr Hutchison admits that he did not so seriously inquire about its origin as he might have done; but began to suspect the dietary about the 18th or 25th of February, yet thought want of exercise might be as much concerned in its production as any other agent. Dr Hutchison afterwards ascribes scurvy on board ship to deficient nutriment as a leading cause, and to mental dejection, and want of active bodily exertion, as accessories, because it less frequently attacks the thorough-bred seaman than landsmen, and wasters who have undergone a sudden change of circumstances and of diet; and is of opinion, that in the Penitentiary, the moral causes which have always existed, operated more powerfully in consequence of reduction of the dietary. Admits, that damp air from wet apartments, or damp beds, is one cause of scurvy; but states, that the scorbutic convicts in the Penitentiary cannot be said to have been exposed to it.

Sir Gilbert Blane has no hesitation in saying, that the disease arose from deficient nourishment as a primary cause, aided by the secondary influence of want of air and exercise, and mental depression from long captivity; states, that he was not qualified to form this opinion, unless from inspecting the diet table; and had he been consulted, he should have loudly condemned it. Admits, that the length of time during which the penitents are kept on the diet, is a material point in the inquiry; that it would not have been injurious in the space of three months only; but in an average constitution, from one to two years might elapse before its effects were discovered. Thinks that, considering the previous habits of the patients, and connecting these with the length of confinement,—the space that elapsed between the introduction of the new dietary in July 1822, and the appearance of the disease in January 1823, was sufficient for its production; the diet was too low; has been conversant for forty years with prisoners of war and others, as convicts for Botany Bay, and is acquainted with their diet, but has seen none so low as that of the Penitentiary; thinks the medical superintendent was quite wrong in saying it was excessive in March 1822; and that the health of a prisoner, who was confined on it for a number of years, would infallibly be injured. Ascribes sea-scurvy to deficiency of food, or bad subsistence.

Mr John Godwin Johnson has been lately officiating in Norwich gaol, as assistant to Mr Scott, who is surgeon to that establishment. States, that about the end of January, or beginning of February, a few cases of scurvy began to appear among the prisoners, in the form of petechial spots; visited the Penitentiary on Saturday 24th May, and saw similar cases, but with ecchymosed blotches; cannot say to what causes the disease of Norwich gaol should be ascribed; the prisoners are allowed to purchase food; and one of the men, in whom the disease appeared, was well known to be abundantly supplied; nor could the influence of the cold weather be said, with greater justice, to affect him.

Sir James Macgrigor cannot ascribe the illness which has prevailed since his visit in March and April to change of diet, but to some general cause affecting the population of the metropolis, as he conceives is shown in the state of health in the House of Correction in Cold Bath Fields, and among the poor in the Poorhouse, and in the parish of Kensington. In the former, the average mortality and sickness for the three seasons of 1820, 1821, and 1822, is about nine, whereas during the three months of 1823, it has amounted to this number; and the same proportion nearly has been observed in Kensington. Regards the immunity of those persons who were in the kitchen as a sort of negative fact, since not the quantity of food alone is to be considered as a protecting cause, but their unrestrained and more cheerful mode of living. Cannot recollect whether the legs of the prisoners, at the visit in February, showed evident marks of scurvy; but has heard that marks of this kind were found, at the examination in March, in between 300 and 400; is not aware that the same disease has prevailed in the out-door population; but observed that bowel affections were very prevalent in the Cold Bath Fields Institution; for instead of the ordinary number of 200 annually, they amounted, in the three months of January, February and March, to 60,—which he regards as an unusually great number. Thinks it is a well-established fact that variation of diet, from soup to vegetables or bread, operates strongly on disease; but is not prepared to say whether it is more conducive to health than any fixed diet which could be recommended. Does not doubt that prisoners, fed on diet wholly farinaceous, may enjoy as good health as those who live on a mixture of bread and gruel with meat; and knows that in foreign prisons, and in religious houses in the Peninsula, the climate of which does not differ materially from our own, the inmates, though animal food is not permitted, continue in health. On the 14th of February, when Sir James Macgrigor visited the Penitentiary, and when the numbers in the female infirmary were much smaller than in any previous or subsequent day, he observed to Mr Holford, that, though the females looked paler than before, he perceived no difference in the countenances of the men. Sir James regards last winter (1822–1823) as very unhealthy during the first three months of the year; that is, in the increased severity and less manageable character of all diseases, but without any sweeping or epidemic illness. Lastly, during the course of thirty years, Sir James has occasionally seen scurvy, but never contagious; but he is by no means prepared to say that it is not so; "for "when a number of men are congregated together, I am al"ways inclined to the opinion that it is difficult to ascertain "whether a disease is contagious or not: the disorder I have "very frequently seen contagious. * I believe it is consonant "to all observation, that, confined in a small space, the conta"gion would be very strong: and if there were a very large "area or space in the open air, the disease would be compara"tively innoxious." (p. 83.)

Dr Andrew Baird ascribes scurvy, as it used to prevail in the navy, to deficient nutriment, whether this proceed from the long use of salted provisions, or any similar mode of liv-

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* There is some mistake here, but we cannot correct it.

ing; to moisture, bad air, want of cleanliness and of ventilation. It is not so prevalent in the navy now as formerly, in consequence of the improved state of the food, more general attention to better clothing, which contributes to cleanliness, and the prevention of moisture by rubbing the decks with sand and stone instead of washing. Has seen the dietary of the Penitentiary of July last; but as he has not visited the Institution since that period, cannot form any opinion on its sufficiency or insufficiency from any alteration now visible. Taking the question abstractly, he might perhaps answer it if he knew the quantities allotted to each prisoner; thinks that it is too small, and in its diluted state must be less nutritious. Dr Baird admits, that as he has never met with a case bearing all the characters of sea-scurvy ashore, he cannot say what effect such a dietary as the present might produce in persons confined for many months; but knows, that the prisoners of war, of whom there were 70,000 in the country at a time, were maintained in good health on 1½ lib. of bread and ½ lib. of meat daily; is of opinion, from the health enjoyed by a numerous part of the community in the United Kingdom, and especially in Ireland, who live without animal food on milk and vegetables, that if a person in prison is supplied with a sufficient quantity of farinaceous and vegetable matter, animal food is unnecessary so long as the health is undisturbed; but thinks that all efforts of the substitution of given quantities of bread for animal food in the diet of prisoners can be determined by experiment only, and that in the Penitentiary animal food is necessary, as most persons there have been accustomed to it. Dr Baird further states, that he cannot conceive, from what he has seen of sea-scurvy in the navy, that it can exist where there is a sufficient portion of animal food and vegetables; has seen the disease produced under a diet of bread, but never in situations where the persons had the free use of vegetables.

Robert Westcott Bampfield, Esq. entered his Majesty's navy in 1799, and served under Sir John Warren and Sir James Hawkins Whitshed, was ten years in the Belliqueux with the present Admiral Viscount Torrington, and has seen scurvy in all its forms, and in every variety, from the slightest to the most violent and fatal, and in almost every part of the globe. As to its cause, Mr Bampfield states, that it must be preceded either by diet devoid of nutritious properties, or by diet of salted meat without a sufficient quantity of fresh vegetables. Has seen it prevail when in harbour, land-locked at Trincomalee in the East Indies, and has seen three cases in the work-house of St Paul's, Covent Garden, London. Two of these were admitted into the work-house with scurvy on them; the third originated in the Institution; admits, that this was the only case among many hundreds in which the disease could not be traced to defective nourishment. Admits, that among sailors who have once had the disease, a disposition to future attacks exists. Mr Bampfield has seen the scale of dietary adopted since July in the Penitentiary, would not have sanctioned it, and should have farther stated, that under such diet, scurvy must have been produced both on account of its insufficiency and its quality. States this opinion under the impression that the soup contains no vegetables; and though it appears that it contained greens, turnips and onions, adheres to the opinion that the animal (vegetable?) matter is insufficient to sustain health.

"It occurred to me at Trincomalee, in the situation I have alluded to, that though the ship's company were allowed one pound of fresh beef per day, such as we have in the East Indies, with a small quantity of vegetables, that in one week after the men were placed on that diet, thirteen cases of scurvy occurred, and I was compelled to write a letter on service to represent that the quantity of vegetables must be increased, and which, on being given, put an effectual stop to the progress of scurvy." p. 87.

Mr Bampfield, after referring, at the request of the committee, to the prescriptions of the dietary, states his opinion, that the soup so made would correspond precisely with that which he had now alluded to at Trincomalee; recals to the recollection of the committee, that during the severe winter of 1794–95, the crews of many ships then lying at Spithead, that were well supplied with animal food, but could not on account of frost obtain much vegetable food, became affected with scurvy; and finally states his conviction, that as much vegetable matter almost as could be put into broth, made in the proportion of $1\frac{1}{3}$ ounce of meat to each individual, would not prevent the disease, and that with such diet scurvy would show itself in this country in about five months, in a tropical climate much earlier, according to his personal experience. In reference to the most proper form of administering food under such a system, Mr B. states that a dietary consisting of liquids and fluids without crude meat, is precisely what physicians call low diet, and is liable, if long continued, to induce diarrhœa. On the influence of alternating or varying the substances used, he admits that the use of farinaceous and vegetable matter six days, and seven or eight ounces of crude meat on the seventh, would have been a better and more salubrious diet; and states the following fact.

"On board a ship, the seamen and midshipmen, or the midshipmen's mess, are placed precisely on the same diet; but as the midshipmen dine out about once a week, either with the captain or officers mess, and obtain one full diet of fresh animal and vegetable matter about once a week; I have certainly, in all my experience, never seen more than two midshipmen affected with scurvy, and that was in a slight degree, in a cruize in the East Indies, in which there were only thirty-six men left free from it, when we arrived in harbour, out of a complement of 490." p. 88.

Mr B. thinks that the diet of a prison like the Penitentiary, taking into consideration the confinement of the parties, and the operation of moral causes, should contain six ounces of dressed meat daily to each man, and four ounces of the same to each woman, with a sufficient quantity of vegetables, and about a pound of bread; and is of opinion that variety of food, or animal and vegetable matter mixed, is requisite to preserve health in a climate like this, as his own observations convince him that scurvy may be induced in all climates wherever full diet of this kind is not used. Is further aware that the Hindoos, whose mode of living has been supposed to contradict this principle, do not restrict their diet to mere rice; for though they do not use beef or mutton, they can at all times almost have milk, butter, cheese, eggs, and even fish.

"And their original stamina has been derived from parents brought up upon that sort of food; the nature of the climate also renders animal food less necessary naturally there; during the greater part of the year the earth is parched up; there is no pasturage for cattle; the cattle being extremely thin and small, nature would seem to ordain that the diet, in those seasons of the year, should consist principally of vegetable matter and the animal matter I have spoken of, for if all ate animal food constantly for one year, they would almost consume all the cattle in India." p. 88, 89.

With regard to the mode of living among the natives of Ireland, Mr B. gives the following statement.

"I have been in Ireland; I think I have visited some of the meanest huts in Ireland, and I never saw an Irishman who was fed so low as on potatoes and milk; I think I have invariably seen that they had hens which laid eggs, I have seen them in general; I have also seen a pig in the house; I have seen a cow generally either in one cabin, or amongst two or three cabins, from whence they made butter and cheese; and I conceive, upon that diet in the Penitentiary, an Irishman might enjoy his health, if you gave him butter, cheese, milk, butter-milk, eggs, and some vegetables, and a slice of bacon now and then." p. 90, 91.

Augustus Granville, M. D., as a naval surgeon has seen scurvy in naval hospitals—a few cases on board two blockading squadrons off Toulon and off Lisbon; has seen it on board captured vessels, in the West Indies, in the French army, on board a large Turkish fleet, in the prisons of Paris, and a few cases in the London hospitals and dispensaries; expresses his belief that every medical man of experience who has seen the disease, will hesitate in giving an absolute answer to the question of the causes whence it originates;—has known it arise not only from want of proper nutrition, but where the diet was too highly animalized, and also where it consisted of mixed vegetable and animal matter to a proper amount. As an example of this mode of formation, Dr Granville refers to three of the Parisian hospitals, in which, during 1817, when he was in Paris, scorbutic diarrhœa arose, in consequence of feeding the inmates on gelatine obtained from bones, according to the process of a chemist, whose name is left blank, but by which we presume is meant Proust; whose experiments prompted Cadet de Vaux, and that active but quixotic speculator, Sir Benjamin Thomson, commonly known by the name of Count Rumford, to make soups and broth in this *economical* style. Dr Granville informs the committee, that in one prison, the Bicetre, scurvy broke out in so severe a form, that the infirmary contained, while he was there, one-third of the population of the prison, which is generally 800 persons; and it was then ascribed to insufficiency of clothing, the dress of the prisoners being a linen jacket and trowsers only, during a very severe winter. Dr Granville admits that the prison in which this took place was rather overcrowded, cold, badly ventilated, and in some parts dirty,—circumstances which doubtless would operate with considerable power as auxiliary morbific agents. These cases of scurvy, which, according to Dr Granville, occur almost annually in the Parisian prisons, are sporadic,—affecting one, two, three, or half a dozen persons, and appear to subside spontaneously. No change of diet is made in consequence, nor does the diet appear to be suspected as the cause. Dr Granville has not visited the Penitentiary, nor is he prepared to say whether the dietary could cause scurvy, till he is informed that the disease of the Penitentiary is actually scurvy; admits that the diminished quantity of food would exercise a very lowering effect on the constitution; but when this is connected with a fact already stated, of scorbutic diarrhœa occurring under a diet entirely animal, he hesitates to give a decided answer; lastly, he sees nothing in the dietary that would induce him to remonstrate against it, or shrink from putting it to trial. p. 95.

P. 99.—The next medical witness to whose evidence we are directed, is Mr John Pratt, resident surgeon to the Penitentiary. The evidence given by this witness is very confused, tautological and vague, savouring strongly of *a little learning*, but showing its danger and its folly with equal strength; and it may, on the whole, be adduced as a happy example of the aspiring style of the half learned mediciner, who strives to conceal the penury of his stores by an eager display of the tawdry scraps of science which malicious fortune, or his more malignant genius, has thrown across his path. Such an amiable and interesting degree of scepticism—such a studied aversion to all common language or every-day terms—such an ambitious tone of science and scientific circumlocution,—we have not often had the pleasure of being amused with; and were we not satisfied that the gentleman was much too serious to indulge in any thing like levity or a jest before a grave Committee of the House of Commons, we should at once regard the whole as a good caricature of some of the medical aspirants of the day for literary distinction. Mr John Pratt ' thinks, about the month of October, he began to pay some little attention,--to notice it in some degree,--a pallidness in their looks; he thinks he did once or twice mention it to Dr Hutchison, that there was a pallidness about the countenances of the people; he did frequently in his journal mention that there was a pallidness and want of strength in their stomachs; and in a letter sent to the committee in the month of September last, it is stated, ' " Although the prisoners appear to be in good health, yet I perceive a *pallidness* about them which to me appears to arise from the present dietary, which I am fearful we shall find shortly will increase, accompanied with a diminution of strength, " &c. and much of the same tedious and profitless gossip. Mr John Pratt, however, does not restrict his talents to the composition of elegant reports. He further indulges in physiological speculation, and entertains the managing committee and the questioning committee with the following choice morsel of gastronomic theory. " I think it is obvious that the " diet, though sufficiently nutritious for a short time, will, when " continued for any considerable period, produce a debility of " the stomach, which having no substance to act upon, must " destroy its muscular action; the food being in that prepared " state leaving very little either for the saliva or the gastric " juices to do, and renders the constitution more liable to re- " ceive disease, and particularly so when the mind is affected " from the loss of liberty; and we find the females are more " susceptible and subject to more diseases than males, " and so on. Now, after this sagacious and oracular piece of medical advice, we think it is by no means likely that a single member of the committee will ever again swallow one mouthful of soup, or any thing less solid than substantial sirloin.

One extract more from the evidence of this witness will show at once the execrable trash, with which it is possible for comtees of inquiry to be satisfied.

" Were statements made to you, on the part of the matron, that, in November or December, some of the females could not keep their broth upon their stomachs?—Yes, certainly, there were.

" At evening or dinner time?—Particularly in the evening.

" Was any thing done?—A change was made, by giving them warm water.

" By whose advice or authority was that given?—By the advice of Dr Hutchison.

" Did you concur in it?—Yes, I did.

" Are you certain he was present?—Yes. On February the 8th I say, " Permit me to state, that I found, both in the male and female pentagons, a diminution of strength and loss of appetite, but more particularly among the females; their countenances are pallid, and they complain of weakness in the region of the stomach; many, I fear, cannot take down their soup. I am fearful we shall, in a short period, find a very great accumulation of disease, arising from debility; and I certainly do not think the present diet sufficient to give them that support which it is necessary they should have." On the 27th of February I have remarked, " There has been a great number of slight attacks of diarrhœa in the pentagons, but which are doing well; this has been both in the males and females; and there is, I apprehend, the scurvy among them, which has made its appearance on their legs, having discovered spots, although the grand mark distinguishing that disease, the wasting and spongy appearance of the gums, had not taken place." p. 100.

Dr Johnson thinks it is exceedingly difficult to determine all the causes that may have combined to produce this complaint; it is highly probable that if defect of nutriment did not cause the disease, it strongly disposed those so fed to the general causes of its formation; he is not prepared to say that this is the sole cause, because some recent cases have taken place in which the men were not more than six weeks in the Penitentiary, and must have therefore been little if at all influenced by the reduced dietary; is not prepared to give a decided opinion on the influence of local situation; the ground on which the Penitentiary is built, appears to have been low and swampy; but the nature of the houses, the pavements, and the draining since practised, would induce him to pause ere he declared the site to be unhealthy; attributes a good deal to the operation of moral causes, and ascribes the general exemption of the officers of the institution as much to the condition of their minds as to their allowance of food. Dr J. regards neither dysentery in general nor the disease of the Penitentiary as infectious.

Mr John Godwin Johnson, cannot say that he has had experience enough to justify any opinion on the infectious or uninfectious nature of the disease.

Dr Burnet thinks that in some instances the dietary appeared to be connected with the scurvy and bowel complaint, but in other instances he could not trace this connexion, and declines giving any opinion on the probability of its being infectious.

It is unnecessary to dwell longer on the evidence adduced regarding the probable causes of the diseases of the Penitentiary. These abstracts may communicate a very correct idea of the light which they have thrown on the subject, and of the degree of certainty with which any one or two of them may be charged with the evils which have taken place in the Institution. It will be remarked, that these evidences are in many places very contradictory, that they are in almost all places vague, and that it is quite impossible to fix on any one agent as the origin of the complaints. This is by no means wonderful, though we doubt not that it will give rise to some scandal among those who are not prepared to appreciate the nature of medical evidence, and especially of that department of it in which the causes and formation of diseases are investigated. In many subjects of medical science, research has been rewarded with discovery, and experience has at length been attended with useful and substantial information; but in this, the physician has much to learn and more to observe, and etiology may be said to be still in its infancy. In the instance before us, there are several circumstances which concur in a peculiar manner to render the inquiry of causing agents extremely complicated, somewhat obscure, and particularly liable to lead to fallacious conclusions. Three of these are very obvious, and may be briefly adverted to. *1st*, We think it is clearly established that scurvy did not prevail very generally, that its existence was at best extremely doubtful, and that it had actually disappeared, while there were many severe cases of dysenteric or intestinal disease. *2d*, We conceive it to be proved that the disease which was most prevalent and most fatal, consisted in inflammation of the mucous membrane of the intestines;—we make no case in this instance of nosological names. *3d*, The continuance of the disease after its alleged causes had ceased to operate, throws a peculiar and unexpected difficulty in the way of explanation.

Suppose the disease were admitted, throughout, to be scurvy, it would by no means be difficult to show, that the causes to which it is ascribed were not sufficient to produce the effect. The first and most obvious, and that which was believed to be the most general, is the diet, and the diminished allowance of the new dietary. Now, though several of those physicians and surgeons have given their opinion, in the strongest terms, that the diet was too slender in quantity, and not sufficiently nutritious in quality, this inference has been again contradicted by others, though not in terms of equal decision, at least on facts equally strong. In proof of this assertion, we refer to the evidence of Sir J. Macgrigor, Dr Granville, Mr Bampfield, and even of Dr Johnson. But the justice of this argument, derived from the dietary, is prodigiously diminished by the history of the progress of the disease; for it appears, from various parts of the evidence, that persons were attacked with the disease shortly after entering the house, and after the dietary had been changed; and long after the food was more abundant, and of better quality, cases of the disease occurred, and were attended with very bad symptoms. (71.)

The next cause believed to be concerned in the formation of the diseases of the Penitentiary, is cold; and unquestionably its influence must be admitted to be very powerful, in a place where confinement renders active exertion difficult, and the regulations prescribe the same degree, in the same manner, to several hundred persons, who probably differ very widely from each other in their habits of exercise, or personal labour. The disease also appeared soon after the commencement of a very severe winter, and increased as the cold continued. But the difficulty again here is, that it did not recede at the approach of warm weather; for in the second Report of Drs Latham and Roget, on the 4th July, it is expressly stated, that "many prisoners admitted since the diet, thought to be injurious, has been changed, and since the weather has become milder, have become the subjects of dysentery; and several of the officers of the establishment most employed about the sick have suffered the same disease." (p. 394.) This, indeed, is one of the strongest arguments, not only against the disease being one or uniform,—against its being scorbutic merely,—but against the alleged influence of the prison diet and the cold season, or general defect of heat in the prison. There is no mode of getting rid of it, unless by admitting, what appears to be the fact, that whatever appearance the disease assumed at its commencement,—whatever petechial spots or blotches appeared on the persons of the convicts,—it was all along, and especially about its termination, a malady of the intestinal canal solely. On the influence of air and the depressing passions, on which much stress has been laid, it is impossible to say much. Their influence on human health none can deny; but we know so little of their peculiar and precise effects, that we think it is quite idle, in the present state of our knowledge, to speculate concerning their operation on disease. We readily admit their disposing influence on all diseases.

These difficulties, with regard to causing agents, are certainly applicable chiefly, if not entirely, to the presumption of the disease being scurvy. But if we abandon this forced and erroneous view of the subject, the difficulties, though not removed, are much diminished. If we lay aside mere nosological terms, and adopt the hypothesis, for we regret to say it can be nothing more, that the disease which was most general in the Penitentiary consisted in inflammation of the mucous membrane of the intestines, or, if the witnesses please, dysentery, for it avails not to dispute about a name, it appears to us that the relation between several of the causes and the effects is much more distinct and conspicuous. We do not, in this case, deny that the slender rations may have had some influence, perhaps by no means inconsiderable, in giving rise to this disease; but this effect appears to be rather secondary than primary, as some of the medical witnesses have represented. Almost all that we know of the influence of diet contributes to show, that it is not a direct cause or agent of disease, that its operation is on the general strength of the subjects, and that it may thus effect in the animal economy changes favourable to the direct attacks of morbific agents. Neither do we altogether deny the operation of inaction, and even want of fresh air, but we allow them only the same secondary influence; we cannot regard them as direct generators of disease, or as any other than mere auxiliaries to its attacks.

It is different, however, with regard to cold, whether alone or combined with moisture. There are two modes in which it is obvious that this agent may operate on the human body; 1st, The general effects of living constantly in a cold atmosphere; 2d, The particular effects of cold applied for a considerable time to the human body so as to exercise a certain change on its functions. The peculiarities of these two modes are most clearly understood by observing the different results which take place in the persons of individuals who live habitually in situations not sufficiently sheltered from the weather, and in the persons of those who are imperfectly clothed in such situations. In the first case, cold may produce direct effects, but, in general, it operates rather indirectly; active exertion, sufficient food, and sufficient clothing generally counteract any injurious impressions. But, in the second case, where the person of the individual is imperfectly or improperly clothed, the operation of cold is more direct, more permanent, more uninterrupted, and less liable to be counteracted by other means. There is no fact, we conceive, so well established as the great influence which sufficient and adequate clothing exercises in preventing disease, and insufficient clothing in causing it; and we think Dr Baird is perfectly correct in ascribing the greater health of our seamen in all climates to the changes wrought in this department of naval economy. Coverings of flannel next the skin are indispensable to those exposed to changes of temperature and vicissitudes of weather, whether in cold, temperate, or tropical regions, not so much in contributing to cleanliness, and in maintaining the skin, its circulation and secretion, in due order, but, through that, in preserving a proper state of the mucous surfaces, and especially that of the intestines. The great influence of its neglect in deranging the state of the intestinal mucous surface, is admitted by all practical observers, both of the diseases of this climate, and of warmer regions; and the fact mentioned by Dr Granville, that the scorbutic dysentery of the Bicetre could be traced to no other cause than that of a linen dress during the rigour of winter, shows how much may be ascribed to it in institutions like the Milbank Penitentiary. The point is, was there proof that this cause existed; that the penitents were insufficiently clothed? It is singular, that while the minutest inquiries have been made at all and sundry about the state of their diet and its effects, about the degree of exercise, airing, kind and quantity of occupations, and even the means employed to warm the pentagons, we cannot discover a trace of any inquiry about their mode of clothing, or the protection which it was calculated to afford them against cold, in a situation where they were precluded from the usual means to which their equals in rank had access. It may perhaps be regarded as a bold and baseless conjecture, but we shall risk it notwithstanding, that had the inmates of the Penitentiary been ordered proper flannel clothing, and especially rollers of flannel round the belly, we should have heard nothing of dysentery, and little or nothing of scurvy. The diet would then have been found good and adequate to its purpose, the accommodation and airing would have been sufficient, and, if any evils really resulted from the site of the Institution, they would have been less likely to be powerfully injurious.

We must not, however, disguise the circumstance, that this conjecture still requires to be confirmed by further proofs. We have already alluded to the second report of Drs Latham and Roget, and have shown that they were compelled to relinquish their original etiology of the disease, and to acknowledge, that the influence of diet and cold weather were not sufficient to explain its continuance in the months of June and July.

" The causes (the exciting causes, as they are called) of diseases are involved in much uncertainty; and when the question is concerning an extensive epidemic, this uncertainty is felt, and confessed to be most painful: still the inquiry into these causes can never be hastily dismissed; for to discover what they are, and to remove them, if they are within our reach, may be essential to the effectual cure of the disease.

" With respect to the Penitentiary, then, while we confess that there has been, and still may be, a cause of disease in operation which we are ignorant of, we are most anxious for the Committee feel convinced, that the fault does not entirely rest with us, that it remains undiscovered, but that the difficulty of detecting it is inherent in the subject itself.

"We beg to state, that opinions respecting the causes of diseases are formed, not from the mere observation of one or two things immediately obvious to the senses, but from a cautious investigation of a great variety of circumstances, and from a series of reasoning upon them.

"It is obvious, that opinions necessarily so formed, are very liable to error; and hence it happens, that no prudent physician ever arrived at conclusions about the causes of a disease, with so certain a conviction that he was right, as not to confess that he might possibly be wrong.

"Numerous cases in the Penitentiary, to which we have already alluded, have seemed to us quite inexplicable, except upon the presumption of contagion. The fact may be otherwise; and authorities (we are aware) preponderate against the contagious nature of dysentery: nevertheless, we have not thought ourselves justified in neglecting the practical measures which the facts before us appeared to suggest, until medical opinion is settled upon this point."—pp. 394, 395.

This admission might justly lead to a long and intricate discussion, and for this we have no leisure at present. But we remark, in the *first* place, that scurvy never was known to be contagious or infectious; and if this imaginary scurvy of the Penitentiary is so, Drs Latham and Roget have the merit not only of framing a new disease, but of making wonderful discoveries in the shoreless but not rockless ocean of medical etiology. In the *second* place, though contagion be admitted as a cause of dysentery—and of this we have very great doubts—the question still recurs, How were the first cases formed?—what contagion gave them birth? Contagion cannot be regarded but as a mere secondary cause of such a disease; and the idea that it is the primary one, though less probable than that which said the same for the dietary, is equally inconsistent with accurate observation and sound reasoning. This point, indeed, can only be determined by the subsequent history of the disease. We have seen, that, in some instances, the turnkeys and other officers of the Institution were affected; and as it is stated that they were not exposed to the influence either of insufficiency of diet, or of those circumstances to which the disease was ascribed, it is not quite unreasonable to look for some agent like contagion. It is likewise stated, that persons recently brought to the Institution had been attacked within a few days (10), *—a period certainly too short to allow either diet or inactivity, or even the other accessory causes, to operate on them with much effect.

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* P. 194. Dr Latham in the evidence of Sir Gilbert Blane.

"Are you aware that three out of the four cases of prisoners who have been in but ten days have got the complaint?—Dr Latham mentioned to me in the other room, such had been the case.

"Does that fact at all shake your opinion as to its being contagious or not?—I own I have never been able myself to ascertain exactly whether it be contagious or not contagious." p. 194, 195.

But unless it could be ascertained, that the persons who laboured under the disease afterwards communicated it to those with whom they were living, away from the site of the Penitentiary, it may be justly doubted to what extent the disease can be said to depend on contagion. It is to Drs Latham and Roget that the profession look for the solution of this enigma; and if, by cautious observation of the subsequent fate or history of those individuals who have suffered it, they dispel the ambiguous veil with which its origin has been hidden, it will not be in vain that they have been charged with the honourable task of investigation.

III. Not the least curious point in the general investigation is the question of treatment. Practically speaking, this resolves itself into two divisions:—1*st*, the most approved and efficacious treatment of scurvy; 2*d*, the best treatment of the disease under which the convicts of the Penitentiary were said to labour.

For a long time, the treatment of scurvy was the most embarrassing and difficult duty of the physician; and a remedy, which should be uniformly efficacious against its ravages, was a desideratum for many ages after it began to appear among European fleets and armies. At an early period it was treated by administering, in substance, various vegetables, which were termed antiscorbutic; as scurvy-grass (*cochlearia officinalis*), water-cresses (*nasturtium officinale*), brooklime (*veronica beccabunga*), celandine (*chelidonium minus*), with cathartics and the addition of aromatic substances to food and generous wines. More recently, when the therapeutic indications were founded in the nature of the alleged causes, physicians rested their curative means on change of diet, and the copious supply of fresh vegetable or animal matter. On a principle not very different, perhaps, is to be explained the use of sour crout, wórt, spruce-beer, and other similar substances, with which we know Cook and others contrived to preserve the health of their crews. But all these means and remedies had yielded to the lemon-juice or citric acid, which, by its uniform efficacy, seemed at length to unite all the desired qualities of a specific antiscorbutic agent. The great success of this remedy, the happy changes which have taken place under its use on the health of our seamen, and the implicit confidence with which it was administered during the late war, warrant our tracing slightly the history of its introduction.

Though scurvy appears to have been known as a distinct disease so early as 1555, when Olaus Magnus describes it as peculiar to persons imprisoned, and the inhabitants of besieged towns, it does not appear that we have any earlier notice of the influence of lemon-juice in checking it than is given by Sir Richard Hawkins, in a voyage to the South Sea in 1593, whose ship's company was attacked with distinct and violent scurvy within three or four degrees of the equinoctial line.* After this authority, we find John Woodall, master in surgery of London, 1636, bestowing the highest praises on it as a known and certain remedy, and recommending it in the strongest terms to naval practitioners.

Yet it is remarkable, and shows the tardiness with which real improvements become known and are adopted in medicine, that epidemic scurvies were allowed to rage in various parts of the world, and in none more than the British navy, for nearly two centuries after, without attempting to use this remedy. So complete was this neglect, that, in 1740, when Lord Anson proceeded on his circumnavigation, no provision of any kind appears to have been made against the disease; and such a thing as lemon-juice appears never to have been thought of. So much, indeed, does this appear to have been the case, that we find Mr Walter, the judicious narrator of the miseries of Anson's circumnavigation, declaring, "that in some instances, both the cure and the prevention of this malady is impossible to be effected by any management, or by the application of any remedies which can be made use of at sea." (p. 113.) The calamities of this unfortunate expedition were, however, productive of one good effect. The impressive description of the sufferings undergone by Anson's crew awoke a general interest not only with the profession, but with the public at large, on the causes and nature of the disease, and the most likely means of controlling its ravages. Among those of the profession whose attention were attracted to this subject, Dr John Lind, physician to the Royal Hospital at Haslar, deserves to be distinguished. It is to his researches, 1753, that we are indebted, not only for the best account of the disease, the critical examination of the accounts of voyagers and medical authors, and the correction of many erroneous notions then prevalent, but for the first clear and rational exposition of the causes of its formation, and the means of its certain removal. There are few works on medicine which are more truly the result of correct observation than this, almost none in which the efficacy of remedial means is so satisfactorily expounded. * Dr Lind, after examining the cures which were reported to have taken place under the use of vegetable diet, or fresh articles of food, or other antiscorbutic remedies, proceeds to say, that "the medicine which succeeded in time, I shall afterwards confirm to be the surest preservative, and the most efficacious remedy, by the experience of others." p. 147. He then proceeds to relate experiments which he made, with various substances or articles of food and drink, supposed to possess antiscorbutic properties; shows satisfactorily that oranges, but especially lemons, remove the disease not only in a shorter time, but with much greater certainty than any other agent that had ever been tried; and after adducing from other authorities, evidence to the same effect, concludes, that analogical reasoning on the efficacy of mere acids is in this case both fallacious and useless; since other acids, as those of tamarinds, vinegar, &c. when actually tried, were found to have no control on the disease.

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* The Observations of Sir Richard Hawkins, Knight, in his Voyage to the South Sea, A. D. 1593. Apud Purchas his Pilgrims. IV. Part, p. 1371.

The strong and satisfactory recommendation of this substance as an antiscorbutic remedy, is not the sole ground on which the merit of Dr Lind rests. It is in rendering the proper acid of lemons accessible at all times, and in all climates, whether in the midst of the ocean, or under the arctic circle. † Not only did this physician himself prepare and preserve the acid juice, but we find that, by his recommendation, several naval surgeons had tried it in this form, and found it completely successful. ‡ It was in 1793, however, before it was adopted in the navy, and two years after, before a general supply was ordered, in consequence of the great success attending its use on board the Suffolk. § Since that time, it has been a constant article of naval medical store, and supplied with the same systematic regularity with which the necessary implements of war have been furnished. Of this, and of the general good effects, the evidence given by Sir Gilbert Blane, and by Dr Baird, afford unequivocal proofs; and while no vessel is allowed almost to quit a port, or go a three months voyage, without a suitable supply of lemon-

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* A Treatise on the Scurvy, in three parts, &c. by James Lind, M. D., physician to his Majesty's Royal Hospital at Haslar, near Portsmouth. Lond. 1772. Edit. 3d.

† P. 162. 164. ‡ 165–168–522.

§ Observations on the Diseases of Seamen. By Gilbert Blane, M. D., &c. London, 1803. pp. 190.

juice, scurvy has been literally banished from our navy. A case of it is a greater rarity than small-pox; and we have no longer to deplore the prodigious losses of men, stores and money, which so frequently occurred about thirty years ago.

With this strong and unequivocal testimony in favour of the antiscorbutic power of lemon-juice, it is singular that, in the College of Physicians of London, a new remedy should start up, the very last from which benefit could be expected, and which has been unanimously execrated by all the practical authors who have ever written on the disease. We allude to mercury. It avails not to retort here our own arguments against the scorbutic character of the malady; the question, in this instance, is the propriety or the safety of administering a preparation of mercury to cure scurvy, and must be examined and decided on abstract principles. So extraordinary and unprecedented is this mode of treatment, that it could scarcely be credited, if it were not gravely defended through the whole course of the inquiry by Drs Latham and Roget, on the broad and irresistible plea of the disease disappearing under its use;—and systematically sanctioned by the signature of a deputation of the College of Physicians.[*] Citric acid, they say, they tried at first, but it griped and aggravated the diarrhœa; and all other means were used, Dr Latham informs the Committee, ere they had recourse to the mercury. We feel much pain in hesitating to admit that all other means were tried; but we cannot easily conceive what time there was to attempt other means justly, between the period of their first visits, and the adoption of the mercurial system; and it is quite evident that the citric acid could not be tried either fully or fairly, when proper means were not taken to discriminate the cases in which scurvy prevailed, if it actually did prevail, and when, at the same time, no adequate measures were taken to obviate the dysenteric symptoms.

But in whatever light the subject is viewed, the exhibition of mercury to subdue an alleged scorbutic disease, if not quite irrational, was singularly inconsistent with every thing established by long and judicious experience. The whole of the other medical witnesses agree in condemning the practice in the most unqualified terms; and Sir G. Blane does not hesitate to say, that if he had found, when he was in office as Commissioner, a navy surgeon who, by his journal, had administered a particle of this medicine in the treatment of sea-scurvy, he should have felt it his duty to move that such surgeon be struck off the list (p. 194.) Now, though we know not that this exertion of authority, however proper it may be in the public service, and as a general principle, is one of which we would approve in matters of science; yet we cannot help regarding the administration of mercury, in such a disease as sea-scurvy, as the most absurd, pernicious, and wanton piece of quackery that ever was proposed or advocated by a body of men. We should indeed like to see it fairly put to trial in the treatment of scurvy in a ship's company, and we doubt not that the most decided proofs would be obtained of its actual virtues. But the profession have not only to wonder and be instructed by this novel mode of treatment; they have a further cause of astonishment in the length to which the mercurial treatment was carried, and the unhesitating boldness with which its salivating powers are defended. It is well known that all good practitioners have ceased to administer this medicine in the liberal doses in which it was once exhibited, and have reprobated, in the strongest terms, those profuse salivations to which such numbers owe the loss of teeth, and of spongy bones: and indeed many appear to have such pious horror of a dose of calomel even, that they would almost as soon prescribe a dose of oxide of arsenic. Yet we find, in the treatment of the penitents, no such half measures, no moderate and gentle mercurial courses, such as Mr Abernethy would recommend, to stimulate the first passages; no alterative doses of the blue pill, or a grain or two of calomel and opium to rouse a torpid liver; but complete salivation, swollen sore gums, and loose teeth. If ever there was a paradox in medical treatment, this is one; and perhaps not the least paradoxical part is, that the mercury did not put a rapid termination to the sufferings of the alleged scorbutic patients. For this, however, it is not very difficult to account. In the *first* place, the disease was *not* scurvy, nor were the patients scorbutic; *2dly*, the patients were wiser than their physicians; and, with a degree of instinctive horror which does them infinite credit, appeared to have swallowed as much of the mercury only as they conceived good and sufficient. It appears, from Dr Baird's account of his visit of June 18th, that he saw many patients who had been taking mercury for seven or eight weeks, and yet in whom the mouth was not affected. These patients were in the habit of throwing away their pills; and even the attendant physicians were led to doubt whether they took them or not. This fact, which is unquestionable, is by no means inconsistent with that of severe salivation in others; for it may be easily believed, without much stretch of probability, that where mercurial pills (with or without chalk) were administered on this extensive scale, extremes would often meet; and that the effects of the too much would be as general as those of the too little. We must not omit to mention, that latterly, when the destruction of pills was detected, inunction was ordered; and few appear to have been exempt from ptyalism.

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[*] Report of Select Committee, p. 9.

The *second* point to which our attention is directed, is, whether the exhibition of mercury was requisite, admissible, or expedient, in the management of the disease of the Penitentiary. This involves two questions; the *first*, the general propriety of administering mercury for the treatment of dysentery; the *second*, whether from the symptoms, and the morbid phenomena of the disease, and from its actual effect, the remedy was proper.

In the practice of physic, and among its practitioners, it is not difficult to observe the prevalence of an empirical confidence in the use of certain remedies, for the treatment of diseases which require patient watching, delicacy, and attentive management; and there are perhaps few remedies on which a greater proportion of this confidence has been bestowed, than mercury. It is undeniable, that it is an agent both efficacious and useful, when adroitly managed; and to none, perhaps, is the successful and bold practitioner more indebted. This very circumstance, however, has led to an indiscriminate, and often injudicious, use of it—to its abuse, perhaps; and every petty mediciner, when he knows not what to do, finds a ready resource in Mr Abernethy's blue pill, or in alterative doses of calomel. Of late years, though its use is much less general among judicious practitioners than formerly, it has been getting a sort of factitious and ephemeral reputation among those who have been in warm climates, many of them very worthy and respectable persons, but not a few deeply tinctured with the empirical measures of the mercurial physicians. We do not pretend to explain how it has got any reputation in the treatment of dysentery, though it is most certain that several surgeons, chiefly those attached to the naval service, have employed it in this disease with favourable results. The plea on which they generally defend this kind of practice, is, that dysentery, in these regions, is connected with a morbid condition of the liver, and the unanswerable argument, that the disease disappears as salivation advances. Now, it is easy to perceive that it is next to impossible to reason on such facts; and all that the rest of the world can possibly do, is to submit, in tacit acquiescence, to the oracular decisions of the mercurialists.

More recent, and we have reason to believe, not less accurate observations, have shown, that the disease does not in every form of dysentery depend on derangement of the liver, and that the symptoms do not disappear as salivation proceeds. The complaints to which the name of dysentery has been applied are known to depend in almost all instances on inflammation of the mucous membrane of the intestines, generally of the colon, but sometimes of the ilium; and unless this inflammation be subdued, it is unreasonable to expect to cure the disease. Hear what is stated by an author whose practical knowledge of the subject entitles him to some confidence.

"In compliance with the very general, and, in many cases, well-founded, partiality for this medicine, which exists throughout India, we were induced to prescribe it, to a very large extent, in the first cases of dysentery which occurred after our landing at Prince of Wales's Island, where the regiment disembarked in September 1807. Dysentery soon occurred here in a very serious shape; and the exhibition of mercury, to the extent of producing profuse salivation, was then a very general practice in the island in all cases of flux; and in the dysentery of natives and old residents in India, it might have been a very successful practice, but was certainly far from being so amongst the description of men we landed there.

"The dissection of every subject who died of dysentery in the regimental hospital at Penang (with one solitary exception), proved the disease to consist entirely in an inflammatory affection of the large intestines, without a trace of disease in the structure of the liver. The consideration of this circumstance, and the reflection that mercury, when given for the cure of other complaints, *is apt to produce some of the very symptoms which are here so distressing,* soon induced us to be more circumspect in the use of this powerful remedy. *Pains in the bowels, griping and tenesmus, mucous and even bloody stools, are not very unfrequent occurrences from the extensive use of mercury;* and this medicine is therefore, in my opinion, not very likely to be useful in removing them. I can readily conceive, and indeed know, that in cases of a protracted disease, where the discharges from the intestines degenerate from pure blood and mucus, and become more of a diseased nature, that there is no remedy so much to be depended upon for the restoration of healthy secretions; but in the pure inflammatory complaint I am now speaking of, mercury can seldom be useful." *Ballingall,* pp. 76, 77.

We regard it as very difficult to evade the force of this argument; and unless it could be proved that inflammation of the intestinal mucous membrane is actually subdued by saturating the system with mercury, we do not conceive that the arguments of the mercurial physicians are of any weight whatever. That there are disordered states of the intestinal tube, and especially of its mucous membrane, which are capable of being removed by judicious administration of mercury, we do not deny; but we cannot comprehend the structure of that mind which supposes that a violent and general inflammation extending over a considerable tract of intestine can be subdued by an agent which we know under certain circumstances will induce such inflammation.

This, however, is not the only mode in which mercury has been exhibited to cure dysentery, nor has its defence been confined to the mere plea of favourable termination. Very large doses of calomel, amounting to a scruple or more, are by some practitioners exhibited repeatedly, and made to traverse the greater part of the intestinal tube. On what plea this can be defended we never could comprehend; for it is certain, that of a substance such as calomel which is insoluble in the saliva and gastric fluid, a sufficient dose is as good as four times the quantity, unless it is wished to excite salivation; and we are not aware that this is the chief object of the *scruple dose* practice. Its efficacy and safety as a purgative in dysentery we should doubt extremely. In short, very few skilful or experienced practitioners look to mercurial preparations with that doating confidence which was once so freely given it; and we doubt not, that as dysentery becomes more perfectly known, and the morbid condition of the intestines more correctly understood, mercury will gradually sink into disrepute, and be at length utterly proscribed.

When we apply these observations to the treatment of the enteric disease of the Penitentiary, we find that they admit of no modification worthy of notice, and that, so far as it is possible to judge from the symptoms, from the morbid changes shown by dissection, and from the view which we have given above of the nature of the disease, the exhibition of mercury was not only useless but injurious. In the disease of the Penitentiary, there were the same inflammatory process, and the same changes of structure which occur in dysentery; and we have above shown, that there is not the slightest ground for believing in any connexion with scurvy, or in any other peculiarity. Setting aside, indeed, as we have already done, all nosological distinctions and scholastic terms,—call the disease, as we sincerely believe it to have been, *inflammation of the intestinal mucous membrane*, what treatment would sound principle and correct experience recommend? Would it justify the administration of mercury? would it eulogise salivation severe and long continued? or would it lavish its encomiums on that treatment which neglected local depletion, and the subduing of local inflammation? The only answer which the investigation furnishes to these questions is that of the disappearance of the disease under the operation of the medicine. But was this uniformly the fact? Did all the cases which underwent mercurialism get well? Did none else get well, or would they not have recovered under other means? The truth is, that the rate of recovery, and the number of recoveries, by no means confirm the efficacy of the remedy, and would prevent us from placing in it that confidence which is so loudly demanded for it by Drs Latham and Roget. The long time during which the disease, if in any degree severe, was under treatment, amounting to eight, ten, or twelve weeks, in most instances (Minutes, pp. 242, 244), the frequent relapses, and even the number of deaths, are sufficient to show that mercury was neither a speedy nor an efficacious mode of treatment. We request attention to the following statements.

We find, from the evidence of Dr Granville, who appears to have taken much pains to place the matter in its true light,—that as Drs Latham and Roget had no personal experience of the disease on so large a scale, they proceeded with cautious steps at first; that after perceiving that the treatment proposed by the medical superintendent was unavailing, they determined to try the exhibition of mercury in combination with chalk; that in the course of their trials, while a female patient accidentally became salivated, the evacuations diminished in number.

" From that moment directions were given for a certain quantity of mercury to be given so as to produce salivation; and such was the persuasion that nothing but salivation would do, that where the mere taking of the internal mercurial preparation did not succeed in affecting the mouth, mercurial frictions were directed to be employed. The quantity of mercury combined with chalk, which is a preparation very well known to the medical profession, taken by each patient, has been as much as five grains every two hours, which is equal to thirty grains of the preparation in the twelve hours, consequently to ten grains of oxyd of mercury during the same period; the quantity of mercurial ointment employed for the purpose of friction, was not stated."—" Of course the Committee will not expect me to say, whether I am prepared to state what the consequences were of this treatment; I can only repeat, that I found 440 or 450 patients on the list under medical treatment; that in most of them the disease had continued eight, ten, or twelve weeks, (I beg that that expression of mine may apply to the number of cases I examined, my attention being directed to those who had been the longest time there, and continued ill, notwithstanding the use of mercury, and the use of mercury was simultaneous with the whole time I have mentioned); that great complaints of relapses were made by some of the officers of the Penitentiary; that a large number of the patients had their mouths ulcerated or salivated to a high degree; that many of the latter are in bed, better with regard to their bowels, but still in bed and not able to leave it; that none have been discharged since the first of June, according to the statement made to me by Mr Pratt; that thirty-two appear to have died since January, which gives a mortality of seven in the hundred; and that of fourteen or fifteen that have been examined *post mortem*, according to Doctor Roget's communication with me, the intestines were found in a state of ulceration in the majority of cases." p. 244.

These statements are, we conceive, quite conclusive on the question, and furnish a stronger evidence against the use of mercury in the disease of the Penitentiary, than all the *a priori* arguments which we could suggest, or all the testimonies of authorities which we could adduce. It is, therefore, with much pain and regret that we hear Dr Latham make the following confident, but somewhat ungracious, and we had almost said petulant, appeal.

"After the hinting and hesitating and disapproving that has proceeded from our learned brethren, for so many days past, we think it but right that we should state our unanimous conviction upon this matter; and the unanimous conviction of my colleague and myself is this, that if we had not treated this disease upon general principles, and that if, in particular, we had not pushed that one remedy of mercury to the full extent to which we have pushed it, every one of the individuals who have been affected with dysentery in the Penitentiary, would have inevitably perished. We stated it as the result of our observation, that there are certain dysenteries (and the dysentery of the Penitentiary is one of them) which are as certainly controlled by mercury as that disease is certainly controlled by it, for which mercury is a reputed specific; that the symptoms of this disease will as certainly disappear, or are abated, when the mouth becomes affected, as venereal sores disappear or amend under the same circumstances; and this is what we have witnessed, with a very few exceptions, in the Penitentiary. Further, I would state, this is no new opinion, for I must be allowed to observe, that we know (if some gentlemen who have been examined here do not know) that this remedy has been employed for ten or fifteen years by the most intelligent medical practitioners in every quarter of the globe, for the cure of this self-same disease; therefore, when it is hinted that this remedy has never before been employed for such a purpose, we can only say, that if indeed it never had been so employed, we necessarily become entitled to the high reputation of great discoverers in physic; to which reputation, however, we resign all claim." pp. 241, 242.

Surely Dr Latham is aware of the distinction between the reviver of an obsolete remedy, and the discoverer of an unknown one,—between the condemnation of what is bad, and the commendation of what is good. No person, we believe, supposes, that mercury in dysentery is a novelty; but, we believe, few will deny that it is a medicine justly exploded and reprobated on account of its injurious effects, and because other means more appropriate are within the reach of the physician. We trust that the Committee of Inquiry were duly impressed with the weight of the following plain truths advanced in Dr Granville's evidence.

"The practice generally followed in cases of disease of the bowels is such, that any substance irritating, and likely to increase the action of those parts, may be suspected to be the cause of ulceration, if such ulceration be found on dissection; and I consider mercury in large doses, producing salivation (it is my individual opinion, which I believe is supported by great authorities likewise), as an irritating substance, particularly in the state in which it is given, the metallic oxyd.

"Are you connected with the publication of any medical journal, in which this subject has been treated on?—I have been sole editor of the London Medical Journal for nearly two years up to 1822, when I was obliged to give it up, from my occupations not permitting me to continue it; and I have had occasion to discuss and review books on the subject of diarrhœa and bowel complaints in warm climates, and as it occurred in large armies, that have come before the public, as well as systematic works upon diseases of that nature.

"Has Dr Johnson, who gave evidence here the other day upon this subject, himself conducted any review, or written upon the disease and its remedies?—He has not only conducted such a journal, but conducts it at this moment; and he has also written upon the diseases of the parts in question, as well as upon the diseases of tropical climates, in which that subject forms a very material point of consideration.

"Have you any opinion of his upon that subject?—It is not easy to give a precise answer to that question, for I, like many other persons who have read successively, not only his professed works upon the subject, but his opinions as a reviewer also, thought that they appeared to vary so frequently and so much, that the Committee will see how difficult it would be for me to state precisely that I understand his sentiments upon the subject; and I was not present at his examination, therefore had not the benefit of it to shape my answer.

"Then, if you were told that Doctor Johnson had strongly approved of the practice as adopted in the Penitentiary, would you have considered that opinion as conformable to the opinions given in works of which he was the avowed editor, though not conformable to the reviews of which he might be the writer?—Certainly conformable to what he has himself recommended, and with the practice, indeed, of which he considers himself to be nearly the discoverer. In one of the editions of his work on derangement of the liver, he expressed an opinion, which opinion, however, he altered in a subsequent edition, having recommended twenty grains of calomel in diseases of the bowels and dysentery, two and even three times a day in the one edition, and only one grain six times a day in the other; there being the difference of a year or two between the two editions. Indeed the practice has been since known under the name of the scruple-dose-practice of India.

Is the Committee then to understand, that you object to the use of mercury in this disease?—I wish the Committee to understand that I certainly do; and I will assign a very few reasons, for without them, I consider any personal opinion of a man of no weight like myself, might as well not be uttered before this Committee. I object to the use of mercury in the doses alluded to, and with a view to produce salivation, in the disease under consideration, (for I wish to be understood as not condemning the occasional, but the indiscriminate use of it as a systematic plan pushed to salivation); because there are none of our great medical authorities to which we are in the habit of looking up for a guide, that mention the propriety of using it; nay, some of them deprecate it as very pernicious. I also object to it, because the very respectable writer to whom allusion has just now been made, has himself recommended strongly a systematic work lately published upon the subject of irritative diseases of the bowels, &c. in which every thing likely to irritate, or produce the action that mercury produces, is condemned; and in which mercury is scarcely mentioned. I do object to mercury, particularly when pushed to salivation, because I find it very difficult to comprehend, how any disease in which, when it proceeds to its worst period, even under the boasted treatment of mercury (as it appeared in the dissection of some who died in the Penitentiary) the lining membrane of the bowels has been found ulcerated, is to be cured by *producing* ulceration of another part of the same membrane." pp. 244, 245.

Dr Granville goes on to state, that the injurious effects of this medicine in dysentery are clearly proved in the case of the crew of the Admiral's flag-ship, at the St Helena station, in which the surgeon of the Lemon Valley Hospital gives the most decided testimony that it is unavailing. In a subsequent appearance before the Committee, this witness further informs them, that this disease was considered as scorbutic dysentery, and was therefore applicable to the question before them. Unquestionably, if there was one reason stronger than another against salivation, it consisted in this alleged character of the disease.

We find various strong testimonies against the scruple-dose practice, adduced by Dr Granville, from the reports of dysentery in the armies of the Peninsula by Dr Somers, from those of Sir James Macgrigor, &c.; and various facts and authorities against the opinions of Dr Johnson; all establishing the same conclusion, that salivation is useless, and the scruple-dose practice injurious, in every form of intestinal disease. It is, however, quite gratuitous to waste more time in proving what must be so obvious, and we dare say our readers are perfectly satisfied of the true merits of the question. We should not certainly have examined it so much in detail, had we not perceived that opinions very inconsistent with sound pathology were brought forward, under auspices sufficiently powerful to give them great weight; and had we not been apprehensive, that the impression thus made is as injurious to the character of the profession, as it is deceiving to a Committee of the House of Commons. We know not how far our humble efforts may counteract these pernicious consequences; but the consciousness of having done our duty, and having spoken with sincerity, will at least enable us to leave the question as we have stated it, in the hands of the profession, and of the public. We have ventured to differ from some medical authorities of first rate eminence, both on the nature and on the causes of the disease of the Milbank Penitentiary; and, what is worse perhaps, we have expressed our disapprobation of the treatment in strong and unqualified terms. For these faults, if such they are to be deemed, we have only to plead sincerity of intention, and the utmost indifference to every cause unless that of truth. We have no interest in either party, and have reason to believe that we are completely unknown to all concerned, and it is therefore unnecessary to make any further apology for the views which we have taken of the question before us. Lastly, we doubt not, that if these pages are honoured by the attention either of the physicians of the Institution, or of their friends, their indignation will not be inconsiderable, at the presumption manifested in advocating the opinions which we have advanced. We are neither fool-hardy enough to censure capriciously, because it is generally believed that censure of the eminent is popular;—nor are we wanton enough to gratify this plebeian appetite at the expense of truth;—nor are we craven enough to fear the expression of those sentiments, which, however they may affect the feelings of the individuals concerned, are the result not only of observation and reasoning, but of attentive and impartial examination of the subject. We could have wished that this avowal had not been requisite; for it is unpleasant to speak of ourselves at any time; and it is tenfold more painful, when it is requisite in defence of our opinions and medical principles. It is wisest therefore, we believe, to put a period to the discussion, and to close an inquiry in which we have had so little to applaud and so much to blame.

II.

Transactions of the Associated Apothecaries and Surgeon-Apothecaries of England and Wales. Vol. I. London, Burgess & Hill; Edinburgh, Black. 1823.

A VERY memorable event has occurred. An extraordinary rise has taken place in the price of stationary; common-place books, amanuenses, and all the apparatus of writing, are in great and unusual requisition; and any knowing spectator, who will lounge along Little Queen-street, where Samuel Gosnell's press is heard working, will perceive, from the frequent and hurried traverses of printers' devils, that the womb of the press is teeming with an unusual birth. Such were our meditations some months ago, and behold the result. The Worshipful Association of Apothecaries and Surgeon-Apothecaries, after having emancipated themselves from the degrading trammels of trade, or receiving an equivalent in pounds, shillings, and pence, for a certain number of pills, powders, draughts, &c. have long been making efforts to signalize themselves as a literary body; and behold they have ascended the heaven of their invention. Here is an octavo, consisting of 160 pages, numbered in Roman letters, and 420 in the Saracenic fashion, making altogether 580 good and true statute pages of letter-press, in which the literary labours of the Worshipful Association are stamped in fair and legible characters.

The courteous reader, or the curious reader, or the contemplative reader, whose bosom is smitten with a passion for tracing the causes of things, who delights to behold the great events which not unfrequently result from trivial or unimportant circumstances, will be interested in tracing the origin of the present work. The fanaticism of Peter the Hermit whitened the fields of Palestine with the bones of the warriors of Christendom; a cannon ball, at the siege of Pampeluna, led to the formation of the most powerful religious and political institution of Europe; the playful tricks of the children of Zachary Jansen, are said to have suggested the discovery of the telescope and of the new planets; and the curious speculator may trace the Association of Apothecaries to an excessive rise in the price of glass.* It is not literally understood, we presume, that there were not one or two other accessary means, such as committees, reports, petitions to the House of Commons, and, we believe, an act of Parliament; but these were mere collateral means, the base instruments of a noble purpose; and it is clearly in evidence before us, that the primary, grand, and original agent of the whole business was the unusual and unprecedented price of phials, and other forms of the vitreous manufacture. It was on the 10th day of January 1815 that the final legislative act was passed, by which this Association was constituted on its present footing. Meetings were held, resolutions were passed, and reports were presented; numerous subjects occurred for consideration and inquiry; and the schemes of the Worshipful Association were canvassed in lengthened discussions which too often led to nothing. One purpose, however, they zealously pursued, and have never lost sight of till its accomplishment. The science of Medicine was in a state of the most deplorable neglect. The Universities were relapsing into the savage barbarism of the fourteenth century; the London schools were all gone mad with materialism, phrenology, and other such unholy pursuits; the physicians and surgeons of London and Edinburgh, instead of collecting cases, were all to a man busy in collecting votes, or collecting guineas; professed societies had long been in a state of slumber from which not even the dog of the seven sleepers could have roused them; and in short, the medical sciences through the whole empire were in the most desperate condition imaginable. Matters having come to this irretrievable state, some great effort was requisite to preserve the knowledge of the art of healing, to rear up a race of Themisons, who might supply the place of their indolent predecessors with unknown alácrity and more trustworthy zeal. But by whose hands was this patriotic achievement to be accomplished? How was the effort to be made, when all the constituted scientific or academical bodies were so lost to their own honour, and so utterly deaf to the calls of duty and the interest of their fellow-creatures? The worshipful Association of Apothecaries resolved that their talent should not rust in sloth; and, with a degree of courage which does infinite credit to their public spirit, they have assumed the panoply of learning, and have come forward boldly to essay what men may do in the advancement of science and the service of mankind. They stepped into the breach, and sustained the falling temple of medicine.

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* Introductory Essay, p. vii. & viii.

The Transactions of the Associated Apothecaries and Surgeon-Apothecaries of England and Wales contain twenty-eight articles by various authors, and on various subjects; and, that their labours might commence at the right end, the first two are of a general nature, and are devoted to the consideration of the leading and fundamental subjects of the state of the medical profession, and the means of improving it. The field is extensive, and the authors, we can vouch for it, have traversed it in a most masterly and edifying style. The first, which is termed the Introductory Essay, is stated to be the work of the sub-committee. It comprises "an account of the origin of the Association of Apothecaries and Surgeon-Apothecaries, and of their objects;" and furnishes a series of documents illustrating the mode in which they were instrumental in obtaining the Apothecaries' Act, as well as the causes of its defects. These matters occupy ninety-two pages, and cannot fail to be extremely interesting to all and sundry members of said Association. To the world at large, we do not know that they will prove very attractive; for we have remarked, with much regret, that all the subjects which are nearest and dearest to the members of the medical profession, and especially to the Association of Apothecaries, are, by some strange fatality, matters of great indifference to all others.

The subsequent part of this Essay is intended to be of a more general nature, and we trust that its merits will be duly appreciated by the medical and the literary reader. It is "dedicated to an exposition of the Committee's views of the present state of medical practice in this country; of its defects and their causes; of the best mode of obviating these defects; and, lastly, of substituting, for the existing limited views of medical science, such a more comprehensive system of study as may at once tend to raise medicine to the rank it ought to hold, and will at some period hold among the exact sciences; *and force from society that consideration for its professors which will certainly afford the relief they claim.*"

This philosophical excursion is at once commenced by a quotation from the 3d vol. of the Medical Intelligencer, which gives a very deplorable picture of the state of English Medical Literature.

> "We have scarcely any works which treat on general pathology—none original on anatomy—no general ones on physiology, and but few on detached parts of that branch of science—no disquisitions on particular diseases, founded on a laborious and extended comparison of those diseases during life, with the morbid appearances found after death—no general expositions nor explanations of the principles on which our modes of treatment are founded; as, why we bleed in inflammation, &c.; in short, few works which treat extensively of diseases, as considered in their only true point of view, that of their connexion with what is called physiological anatomy." pp. xciv, xcv.

In short, the whole profession has been descending headlong to perdition; and it appears quite certain, that diseases are not cured though doctors are summoned, nor patients recovered though medicines are prescribed. These manifold evils are ascribed by the sub-committee to the defective state of English medical theory, which is very wisely distinguished from hypothesis; and sundry words of exhortation are delivered to impress the necessity of collecting, recording, and comparing facts, of distinguishing fact from opinion, and cause from effect. This however is not the whole. It is shrewdly insinuated that medical men are bad logicians, that a work on medical logic is much wanted, (have they forgotten Sir Gilbert Blane?) and in short that the persons at present destined for the medical profession are only a set of semibarbarous empirics, who are utterly ignorant of the right method of prosecuting its study.

After exposing and deploring these heinous evils, the sub-committee proceed to deliver their sentiments on the means of remedy. Anatomy, physiology, pathology, and therapeutics, they inform us, comprise the whole of medicine; but of all these, physiology is the principal and the most indispensable; it is "the real foundation of medical knowledge; and anatomy is only necessary, because before the functions can be understood, the structure of the part must be examined." civ. This leads incontinently to a passionate eulogy of the science, of Magendie's Journal, and of the French physiological school, the discoveries of which, they inform us, beat those of Home, Philip and Brodie hollow, and are to be matched by none but those of Mr Charles Bell. Pathology next comes under consideration, and we meet with sundry reflections on the nature of disease, whether it depend on change of action or change of structure; and the extreme neglect with which the influence of the animal fluids is now treated by physicians. After proving that a necessary connexion subsists between derangement of structure and deranged action, some judicious observations on the carelessness and incapacity of students and practitioners in observing and noting symptoms follow. To counteract the influence of this unphilosophical spirit, the sub-committee first advert to the ignorance in which the profession is on the minute natural structure of parts, and then enter into sundry abstruse inquiries on the property of the nerves, which they conceive to be a *tertium quid*, which is something we do not comprehend,—on the moving powers of the blood, which they regard as a mystery; on the question of venous absorption, the contractile power, irritability, and sensibility,—on the organs and actions of which they contend that we are profoundly ignorant. Then follows a disquisition on pathological anatomy, a fashionable term which has been recently introduced, and which they decidedly prefer to the antiquated epithet of morbid anatomy; and on the best mode of pursuing its study. For this purpose they introduce an abstract of an article, called in the language of France, *Anatomie Pathologique*, by a man named Gilbert Breschet, in the Dictionnaire de Médecine. This is followed by a short account of a similar arrangement, proposed by J. F. Meekel (Meckel we presume is meant), and the tabular view of M. Beclard.

The peroration is very interesting, and is constructed according to the established rules of the best rhetoricians, and the most able masters of the great instrument of language. After the foregoing able exposition of the defects, the objects and the capabilities of the study of medicine, the sub-committee come to represent some of its advantages as an art. They first advert to the satisfaction which every generous spirit must feel in alleviating pain, healing disease, or snatching from the tomb; and then to that mental delight which is afforded by the reflection, that this is the result of a judicious use of knowledge, varied and complicated in its origin and application. But however exquisite these enjoyments are, it is undeniable that they are temporary and unsubstantial. They are shadows, a trim reckoning, and have no skill in money-making. In a word, they sink into insignificance, when compared with the *main chance*. The sub-committee therefore show a happy combination of scientific zeal, with worldly wisdom, in the following dexterous appeal to this ruling passion of the medical breast.

"A gratification of another description, however, awaits him who prepares himself properly for the exercise of his professional duties. It is a gratification, indeed, of a less pure kind, but it is one which, in the present state of the world, is, to a certain degree, *necessary for a man to enjoy, in preference to the other*, although of an intellectual nature. The *pleasure* derived from *profit* is here alluded to." p. cxxxv.

The application of this ingenious hint will appear anon.

Our sub-committee then proceed to lay down the maxim as an established principle, that all other circumstances of address, manners and moral character, being equal, success depends on professional skill entirely; and, in conformity with this view, they assert, that it is, in all instances, possible to trace failure or success relatively to their respective causes.

"It will be seen in one man, that distinctive personal qualifications, an excellent address, a soothing manner, a real or assumed interest in the welfare of his patients, and, above all, a scrupulous and unwearied attention to the gratifying of patients and their friends by falling in with their views, and thus making the unessential as well as the essential circumstances of the case important, will be amply sufficient to lead even a superficial practitioner to the summit of his profession; whilst in another person, marked defects of manner and of appearance will, if good conduct be present, not prevent him from equally rising to professional eminence, if his acquirements authorise such an elevation. Again, it will be found, that even slight indiscretions with respect to the collateral, or, as it may be called, moral treatment of a case, will often do a practitioner essential injury: so much injury, indeed, that if such want of conduct be habitual, it is of no avail that he has influence, and is recommended most extensively by his surrounding friends—he may gain admission into many families, but he will not retain possession of the ground which he occupies. On the other hand, it will be as often observed, that a sedulous and satisfactory attention but to one case, has been sufficient to introduce a practitioner to many more, each of which has probably, under similar circumstances, formed an equal centre for new acquisitions. In one instance, a young medical man was enabled to trace sixty new cases to the effect which he produced on the mind of the first patient who applied to him on his engaging in practice; whilst, in another, an old established and celebrated physician was seen to lose a most valuable connexion by a very slight personal indiscretion with regard to the mode of his giving a prognosis. It should be stated, however, in this case, that probably the indiscretion would not have produced such an effect, if the prognosis had not been erroneous." pp. cxxxvi—cxxxviii.

We hope that these impressive lessons will not be lost on those whose interest it is to avail themselves of them; and that no consideration whatever will induce a practitioner, whether young or old, to prognosticate recovery in catarrh, or death in consumption. In the other requisites of address and manners, the *elevés* of the Association, we have no doubt, will prove apt scholars under tuition so excellent. The regulation of moral character we leave to the care of the Society for the Suppression of Vice.

The second article of the volume is the first of the Transactions, strictly so called. It is entitled, An Essay on the Education and Duties of the General Practitioner in Medicine and Surgery, by Mr Alcock. If the Introductory Essay professes to be merely a sketch, this is to be regarded as an extended and elaborate view of the subject of education and duties of the general practitioner. The sub-committee open the subject in a general manner, and take brief and rapid views of the whole; Mr Alcock rushes to the root of the matter, views it in every light, penetrates to its uttermost recesses, and leaves no nook of the varied but uncultivated field of medical education unexplored. The prospect is so extensive, or the objects are so complex and confused, that it is not altogether easy to comprehend or trace the course of the surveyor. He begins, as he ought, with the importance and utility of his task, states its objects, and illustrates and enforces, by adducing apposite and striking examples from the daily practice of physic,—in which the ignorance of its professors, or the obscurity of cases, gives rise to numberless mistakes in the treatment of diseases. He then proceeds to consider the motives which are to influence young gentlemen in the choice of a profession, and especially in selecting that of medicine or surgery—enters on the merits of apprenticeships, previous or concomitant education, classical, literary, scientific and professional,—and discusses the comparative merits of different modes of training at considerable length. Professional instruction, of course, claims the greatest share of his attention; and, after considering the means of initiation into the elementary parts, he proceeds to enumerate the courses of study by lectures, hospitals, dissections, and reading,—for one, two, or three seasons, as circumstances may suggest. Anatomy is strongly recommended every season. The lectures of Dr Armstrong on the practice of physic, and of Mr Brande on chemistry, are eulogised; but those of the Colleges of Surgeons and Physicians are damned with cold praise. It may be thought that all this is very tedious and by no means attractive to the unenthusiastic reader. Quite the reverse: It would interest the coldest and most indifferent person in the world, and would command attention and awaken curiosity in breasts which had never thought or felt before. To enliven the dulness of mere didactic dissertation, Mr Alcock introduces a variety of episodes, partly narrative, partly dramatic, which are at once conceived and executed in the happiest style to captivate the budding ambition of the young candidate for medical eminence. The subjects of these interesting digressions are generally derived from the most agreeable part of the practitioner's business,—the substantial emoluments; and they are most appropriately contrived to show that the most accomplished is uniformly the most money-making practitioner. Of this some people, it is said, have doubts; but we hope they will neither show their ignorance nor their rudeness by expressing them.

Having conducted the student to the object of his labours, the diploma, Mr Alcock proceeds to lament the incapacity so generally remarked of observing the phenomena of disease—

gives directions for improving this part of his education, by enumerating the circumstances in the person and functions of the patient which are to be examined—recommends two synoptical views of these circumstances in the diseases of adults and of children,—and terminates in a jeremiade on the uncultivated state of pathological anatomy, and the sacrifice of time which is requisite to render its study productive of much benefit.

The subsequent part of this Essay is on the second division of our author's subject,—the duties of the general practitioner, and is occupied in the discussion of several topics, which are certainly not devoid of interest to the profession at large; but which are infinitely and inconceivably more so to the practitioners on the south of the Tweed, and especially in London, and other places where the distinctions between physician, surgeon, and *routinier*, are so rigorously observed. Mr Alcock adverts first to the history and origin of this distinction, that is, in a general way,—then proceeds to consider the situation of the general practitioner, in relation to the other two orders, —and examines the degree of power communicated by the Apothecaries' act, both in maintaining the rights of its members, and protecting the public against the frauds and impositions of ignorance and quackery. The defects of this act he exposes at considerable length; yet admits that it possesses advantages, the operation of which may be expected after some time; and thinks that a more regular and comprehensive system of education, which has been already insured by it, cannot fail to raise the character of this order of the profession. No circumstance connected with the character of the general practitioner has been so disagreeable, or the source of so much dissatisfaction, as the mode in which his services are rewarded; and none which the surgeon-apothecaries have exerted themselves so much to abolish, as this ignominious mark of their original connexion with trade. It is evident that one of the principal, and, it must be admitted, most reasonable objects of their various projects of reform, is to obtain a more honourable estimation for the services performed, and a rate of remuneration more suited to their qualifications, and regulated by more liberal principles. This subject has not been forgotten by Mr Alcock, who has stated, on the whole, some very true and undeniable circumstances, on the narrow policy by which the whole of this business is regulated.

"The mode of remunerating the general practitioner is not only degrading but unjust; it degrades that education on which his usefulness depends, sets it at naught, and values only the drugs, and not the skill that directed their use. It is degrading, inasmuch as it holds out a temptation to furnish a larger quantity of medicine than may be absolutely required, and thereby exciting distrust; and it is unjust, inasmuch as the reward is not in direct ratio to the ability exercised, but inversely: the skilful practitioner who readily effects the removal of disease receiving the least; and the inefficient practitioner whose want of skill allows the disease to run a protracted course receiving the most. Further, the well-educated and efficient members of the profession are well aware that mere medicine is but a subordinate part of the successful treatment of disease; much must always depend upon the ability to detect and guard against the causes which have produced it: in acute disease improper food, &c. will more than counteract the best directed medicine." pp. 121, 122.

The remedy follows the evil.

"Let the general practitioner then receive a moderate compensation for each consultation requiring the exercise of his skill and experience; and let him freely furnish, without further charge, the medicines that may be necessary to the recovery of his patient, considering them as part of the means of promoting the end for which he is consulted. I should also recommend that the compensation, be it ample or limited, be made at the time of consultation, and be not suffered to accumulate, like an account from a chandler's shop. Even should surgical assistance be afforded, let it, unless in extraordinary cases, be put on the same liberal footing as the medicines furnished, as even the manual aid must be secondary to that exercise of the reasoning faculties which is necessary to render it efficient." pp. 122, 123.

Very sly and ingenious, it must be admitted. The sub-committee first talk, in an under tone, of the necessity of the sublunary but substantial gratification of their appetite for the precious metals,—of the *pleasure* derived from *profit*, as they quaintly express it. Mr Alcock comes out at once with a systematic and regular method of directing a current of riches into the purses of self and friends. The hint of the sub-committee was a mere human longing after the Mammon of ungodliness;—this is an undisguised and unblushing display of the cloven foot, in all its original sin and deformity. It is painful to think, that the *auri sacra fames*—the insatiable craving for gold, should lead the members of the Association to such alarming extremes.

We cannot pretend to give any thing like an abstract of this paper. The author, in the general scope of his argument, is certainly right; and it is almost impossible for any person to be wrong in demanding a more comprehensive and more regular system of education for those persons who are destined to attend to the health of the great majority of the community. It was scarcely possible also for him to be wrong in recommending the long, attentive, and serious study of anatomy and clinical medicine, and the habit of observing the exterior signs of disease in a systematic and watchful manner. It must further be granted, that he has entered into sufficiently minute details in many parts of his subject, and has not only given proofs of a mind well trained in himself, but shows that he is thoroughly aware of the necessity of regular mental training to all who are to be conversant with the practice of physic. Some subjects, however, he has not viewed in the light which appears to us to be the true one, and we are not sure that those to which he has devoted so much attention, are precisely the most important. There is a degree of finical frivolity in the precision with which he delivers his rules for spending the different portions of the day; and some practical absurdity in the partition of time for studying the arteries, veins, lymphatics, nerves, &c. and in the nice division of half and three-quarter hours for sundry occupations. The general tone in which it is written is one which we doubt not is exceedingly well calculated for the meridian of London and its vicinity; and it is no small praise to Mr Alcock that he has succeeded in engrafting so much literature and science on the good old man of Cocagnia. We cannot, however, enter into minute or critical examination of all the merits of Mr Alcock's paper, which must be read to be estimated justly. With the utmost deference to the opinion of the Worshipful Association, we should have doubted the wisdom of placing a paper of this nature at the very head of a volume professing to be the Transactions of an association for promoting and improving the practice of physic. Those who consult or read these transactions, certainly do not wish to learn from them the complaints and grievances of the London apothecaries, or the galling inroads on their practice and professional emoluments by quack-doctors and other unqualified persons, or even the gross ignorance of nine-tenths of the English general practitioners; much less is it desirable to listen to their complaints of inadequate or uncertain remuneration, and their schemes to render this more liberal, prompt, and regular. We do not say that the subject is either improper or unnecessary for consideration; but it is not in its proper place in the present volume. It ought in fact to have been published separately.

Mr Alcock's scheme of education might either have been there or not, though we are inclined to the latter alternative. Most of us know what good and sufficient medical education ought to be; and those to whom this paper is addressed, we conceive, will not learn a single new fact, or have one new idea created in their minds. If Mr Alcock was determined to act the Holofernes, it would have been well to inform us of the actual provision which the Association make for the education of their members, to tell us what course of study and what accomplishments they, in point of fact, require in candidates for admission to their privileges. On the advantages of this system he might have commented with great propriety; but we would recommend that he address his paper not to the Association of Apothecaries, not to the profession—but to that branch of general society from whom the youths destined to recruit the ranks of the great corps of general practitioners are to be derived. It is to parents, guardians, and superintendents of young persons in general that such an exhortation should be addressed. We are well aware that most absurd and erroneous notions prevail on the subject, but the difficulty is to eradicate these notions from the very spot in which they grow. It is useles to lop a branch or crop a few exuberant leaves while the root of the evil remains as firm as ever. A very few observations will place the whole matter in a clear light, and render it quite intelligible even to our general readers.

It cannot be doubted that the constitution and wants of society in all parts of the kingdom, except London, Bath, Edinburgh, and one or two similar cities, require a supply of medical practitioners who can and will act in all ordinary cases as physician and surgeon at least, if not as accoucheur occasionally; and it is equally certain that the practitioners of all provincial towns, or other considerable places, be they graduates of any university, fellows or licentiates of either of the colleges of physicians, or members or licentiates of either of the colleges of surgeons, are, in point of fact, the physicians, surgeons and accoucheurs of their respective patients in such places; and if any extraordinary medical or surgical advice is required, it is obtained by summoning a physician or surgeon from the nearest city in which he is to be found. It is, indeed, on extraordinary occasions only that the physician is thought of in most places; while he is the daily visitant of Grosvenor Street and Berkeley Square. But though this partition of labour and rank be admitted, it does not follow that separate associations should exist for the individuals of each. If this principle be admitted at all, it ought to be admitted in its fullest extent; and we ought to have not only separate colleges of physicians, of surgeons and of accoucheurs, but we ought to have a college of oculists, a college of aurists, and a college of dentists. In London, it is well known, that though the surgeons of hospitals and professed operators candidly decline giving medical advice, or attending medical cases, in which they are conscious a physician would be more useful, yet several of them do not scruple to act as ordinary medical advisers and attendants to their patients. The accoucheur department has generally been left to physicians, whether fellows or licentiates of the College. In Edinburgh, this arrangement is still more unequivocally recognised. Not only is the general medical education so conducted, that the graduate of medicine must understand at least the general principles of surgery; and the surgeon must be familiar with the principles and practice of physic; but the College of Physicians, by recently repealing their bye-laws, which prevented the physician from performing the most trivial operation, in whatever circumstances he might be placed, have enabled their members to do personally whatever they deem requisite for the welfare of the patient, or the removal of his disease. The surgeons, it may further be observed, are, with few exceptions indeed, the ordinary medical advisers of their patients, and practise every department of their profession, except that of midwifery, for which there is *no college;* and the practitioners of which, therefore, take refuge either in the College of Physicians or in that of Surgeons. The Edinburgh College of Surgeons, in truth, by requiring the study of midwifery, and a competent knowledge of *materia medica* and pharmacy, as well as anatomy, surgery, and the other branches of surgical education, in their candidates, unite the advantages both of the London College of Surgeons and of the Association of Surgeon-Apothecaries. Their licentiates are qualified to practise medicine, surgery, and midwifery, in all their departments, and even pharmacy; and the result is, that we hear no complaints in the whole of Scotland about the merely nominal distinction of physicians, surgeons, and surgeon-apothecaries.

To these observations, if they were intended merely to show that an association of surgeon-apothecaries is superfluous, it would be easy to reply, that in London it is otherwise; and as the College of Surgeons take no cognizance of the capacity or attainment of candidates in midwifery and pharmacy, it is requisite that some public authority should be invested with this power. We do not adduce these remarks, however, so much with this view, as to show, that according to the constitution of the Scottish Colleges, the evils of which the bulk of the profession complain in the South, cannot exist. The regulations of these Institutions imply what, in point of fact, is the case, that the general practitioner exists every where; that the distinctions of the profession are in many situations unnecessary or impracticable; yet where they are practicable, they leave it to circumstances, and the choice of the individual, whether he is to restrict himself to one department, or act in all.

But another inference which these observations furnish, must not be omitted. If they do not show that the Association, as it stands, is superfluous, they prove, that the power of domination which it wished to possess over the country, partitioned into districts, is quite unnecessary. Though we do not wish to offer, on this part of the intended constitution of the Association, any opinion which could be said to prejudge a tentative measure, all liberal minds, we are satisfied, will regard it, in the mean time, as the most Gothic and mistaken arrangement that could be devised; and defeating, by its very nature, that purpose for the accomplishment of which they are so eager. The division of any country into so many medical districts, over which the Association is to extend its monopolizing paw, reminds us most accurately of those restrictive measures in social and commercial affairs, against which all enlightened minds have been long struggling, and which it is the credit of the present age to have abolished. It is, in truth, a retrogradation in medical economy; and it is fortunate that this part of the apothecaries' act was not carried.

Lastly, the Association of English Surgeon-Apothecaries, as it now stands, will contain members who will be much more efficient observers of disease, and much better acquainted with the practice of their profession, than could be expected under the old constitution. Still their assistance will be employed on ordinary occasions only; and it will depend on their personal abilities and exertions entirely, whether, by superior knowledge and management of disease, they shall ever obtain with the public that consideration and confidence with which the advice of a popular and skilful physician or surgeon is requested. The Institution must be useful in provincial and small towns,—the only situations, we have reason to believe, in which it previously existed, and consequently in which all the changes now contemplated could not take place. But in London, and in other cities, in which it is obvious that the surgeon-apothecaries feel their grievances most acutely, it may be doubted whether the changes intended can take place without converting the apothecaries into virtual surgeons or physicians. It is evident, that if this be the case, there still must be an order of the profession who are to restrict themselves to the composition and preparation of medicines, whether these medicines are prescribed by physicians, surgeons, or accoucheurs; and whose advice will be requested by the great mass of town population, who are neither sufficiently low to apply to hospitals and dispensaries, nor in that elevated region in which the physician and surgeon are habitual or frequent attendants. Though not particularly anxious to apply the principle of partition of labour in its extremity to the medical profession, we have our doubts whether any individual, however educated and trained in habits of diligence and regularity, or even aided by partners, assistants, and senior apprentices, can attend to that extensive course of professional duty which the surgeon-apothecaries appear to claim. If the subsidiary business of preparing and dispensing medicine is done by the aids to which we allude, how does this surgeon-apothecary differ from the physician or surgeon? He is virtually and truly one or other, or both; and this, we conceive, is the result of the apothecaries' act. It thrusts the apothecary out of his proper path, and converts him into a different person altogether. To this, of course, it would be unfair to offer any objection, if this were the original intention of the act; but if it is actually the case, then what need of an Association? and why do not the surgeon-apothecaries become at once and *ab initio* surgeons and physicians?

Admitting, then, the necessity of general practitioners, the question of education is very easily decided. Practically speaking, the great object undoubtedly is, to render the individual practically familiar with all those usual occurrences of professional duty, which daily take place in such situations as those, in which he is to exercise his profession. It is one of the evils of a profession like medicine, that this is in many instances extensive, exactly in the inverse ratio of the causes which would weigh with a professional person of considerable accomplishments, and of extensive education. The common diseases of adults in towns, the smaller, sometimes considerable operations, accidents of no mean importance, midwifery and the diseases peculiar to parturient women and children, present a field both extensive and varied, yet not boundless. It is certain, that it is practicable for individuals of moderate capacity and industry to master them; and it is on this possibility that the education of the general practitioner is founded. But another question is suggested, whether the kind of occupation in which the general practitioner is to be engaged, will repay the expense of time and money which must be incurred in a course of education so extensive as that which is projected by Mr Alcock. We will not espouse that narrow and calculating policy which refuses to expend, especially on the business of education, what is not sure to be repaid by the gains of the profession to which the education is preparatory. But it is evident, that unless a convenient limit be fixed to this by statute, many individuals who would make excellent general practitioners, might be excluded by the expense incurred, and the time spent in education. It is, therefore, between the two extremes of sufficient instruction to insure qualification and public safety, and moderate expense of time and money to the individuals, that this medium is to be found. That the general practitioner ought to possess the advantages of what is termed a liberal education, is, we conceive, quite undeniable. But it would be at once absurd and unreasonable to demand that he should obtain this at a University. If he is trained in the ordinary manner at a public school to the classics, and is initiated into the elements of geometry and physics under competent instructors any where, it is all that is requisite. His professional instruction ought to be accurate as far as it goes; for it is much better to know one or two subjects accurately, than to have a general and vague acquaintance with ten. For this purpose, there can be little doubt that he ought to be well versed in anatomy, the management of the common surgical diseases and operations, midwifery, pharmacy, and the general principles of physic. Other departments it would be well for him to know; but their study would at once consume time, which to him is invaluable; incur expense, which is also not to be rashly done; and perhaps divide and distract his attention, which is the thing most of all to be avoided. It is astonishing how much multiplicity of object distracts attention, and sometimes promotes indecision,—a quality of mind most inconvenient to the *routinier.*[*] His whole time and faculties should be directed to the practical and necessary parts of the profession,—the scientific and ornamental he may afterwards study, if leisure permit, or inclination suggest. With such cultivation, we could easily conceive a mind of ordinary powers capable of observing the phenomena of disease faithfully—of administering those well-known remedies, about the operation and efficacy of which all the profession are agreed,—and of accomplishing, by attentive management, what is too often neither done by skill nor extensive learning. A practitioner possessed of such a mind, is fitted for all that is requisite in daily practice; and while he shuns, at an equal distance, the rashness of fashionable empiricism, and the folly or weakness of fashionable hypothesis, neither his time nor his accomplishments are of that precious nature, that render his services too expensive to the great body of the community.

After this long digression, occasioned by Mr Alcock's paper, we proceed to notice, very shortly, the other communications to the Society.

The first of these is a case of chronic inflammation of the pericardium, terminating in adhesion, by Dr James Johnson. The only peculiarity in this case is, that the inflammation of the pericardium was connected with rheumatic affections of the joints. These cases are not uncommon. Death was caused by effusion of blood in the posterior lobe of the brain.

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[*] Reil on the Education of Medical Routiniers, Med. and Surg. Journal, vol. ii. p. 477.

The History of a case of Spontaneous Rupture of the Uterus, in the seventh month of pregnancy, by Mr Charles Shillito, Member of the Royal College of Surgeons, &c. is a good example of this kind of accident. The rupture had taken place about the lateral part of the neck of the womb; inflammation, terminating in sloughing, had succeeded; so that the left side of the *os tincæ*, of the *cervix*, and of part of the body of the womb, had been removed by this process, and a direct communication formed between the cavity of the womb and that of an adventitious sac in the abdomen, in which the fœtus had lain. This sac appeared to have been formed by membranous adhesion, effused by the peritoneum. Aphthous sores were found in the gullet and stomach. The dissection does credit to the descriptive powers of Mr Shaw.

In the 5th article, Mr Charles Thomas Haden gives an account of several cases, in which various substances, in several instances articles of food, taken into the stomach, had given rise to symptoms of poisoning. Two cases of poisoning by the berries of deadly nightshade *(atropa belladonna)*, recovered under the administration of an emetic and purgatives. The substances suspected in the other cases are, orange, roast pork, pickled pork and greens, beef's heart, mutton chop, and cherries. It may be doubted how far these agents were the sole causes of the symptoms in each case. The last is a case of apoplexy, succeeding to a blow caused by striking the head violently, four days before, on a piece of wood. Coagulated blood was found in the lateral ventricles, and in the 3d and 4th, and in the cerebral substance of the left hemisphere.

The 6th article is a case of injury of the upper jaw, by Mr John Powell, member of the Royal College of Surgeons. The two front incisors were beaten out, the alveolar processes broken, and two incisors driven up into the right nostril, where they continued four years till removed by Mr Powell.

The 7th article is on the treatment of painful affections of the nerves, arising from local injury, by Mr George R. Rodd, member of the Royal College of Surgeons. The remedy is extract of nightshade *(a. belladonna)*, used internally, and as a local application. The 8th is a case of aneurism of the aorta, and dissection, by Mr John Hunter, of the College of Surgeons.

The 9th, entitled, "Abstract of Communications from the Country, on Medical and Surgical Attendance on the Parochial Poor; in a Letter from Robert Masters Kerrison, M.D.," may be very attractive to the members of the Association; but it possesses no claim to be known beyond the local precincts from which the cases are derived.

Mr David Evans of Belper, Derbyshire, is the author of the 10th and 11th articles. The former contains a series of cases of bad practice in midwifery and surgery among uneducated practitioners. The latter is a case of narcotism, by 1½ ounce of laudanum and half a pound of gin. As half a drachm of ipecacuan produced no vomiting, the narcotic and the *blue ruin* were extracted, by pressing the contents of the stomach through a tube previously introduced. Partial recovery took place; 6 ounces of blood were taken from the temporal artery; an emetic of ipecacuan, and tartrate of antimony was succeeded by vomiting. Death took place, however, on the 6th day after, from supposed inflammation of the lungs. No examination.

The 12th article, on Venous Congestion, by Mr Thomas Sandwith, contains speculations and cases, but nothing very peculiar.

The 13th, by Mr Newnham, contains an account of a case in which a still-born child was expelled from the uterus in the eighth month, and fifty-nine days after, a healthy male child at the full time.

In the 14th, Dr G. Wallis, of Emanuel College, Cambridge, gives an account of the efficacy of the meadow-saffron (*colchicum autumnale*) in cases of peripneumony, pleurisy, hydrocephalus, and diabetes. The last case, though disfigured by speculations, is good and instructive.

The 15th and 16th, by Mr Thomas Haden of Derby, treat of subjects closely allied, and might have been contained in one paper. The subjects are, distortion of the feet, and club feet. The nature of the former kind of deformity will be understood from the following singular mode in which the author commences his account of it.

"A gentleman was met walking in the street to-day: he was lame and awkward. His head and shoulders and elbows projected backwards, his abdomen protruded, his back sank inwards, his toes were turned outwards, and it seemed, when he walked, as if he had no motion in any of his joints. The ankles were particularly rigid, and he had a swing of the body in his walk which arose from each hip being projected forwards when he stepped out, as if to counterbalance the want of motion in the ankle joint. The appearance of this gentleman was really ludicrous; for he was a lusty man, and the uprightness of his figure, his projecting stomach, his elbows immoveably fixed to his side, or rather projecting backwards, and his shuffling gait, were at once peculiar and almost ridiculously inelegant.

"When the attention was directed to the foot, it was seen to be broad and flat; and, doubtless, on a closer examination, the arch of the foot would have been found to be almost entirely lost.

"This gentleman's appearance afforded so good a representation of the symptoms which mark the existence of this species of deformity, that it will supersede the necessity for any further description of the effects which it produces on the motions of the body. Otherwise, let any person try to walk with a stiff ankle and his toes turned out, and he will understand what is attempted to be described here." p. 253.

The structure on which these changes depend is thus explained.

"It is presumed that the giving way of the arch of the foot is the cause of this species of lameness; because, on examining such cases, the foot is flattened, the arch underneath is to appearance lost, and the whole limb deprived of its symmetry and fine appearance; for the ankle is too much lowered, and the calf of the leg is is in a great degree deficient." p. 253.

There is nothing very peculiar or new in the pathology of these kinds of deformity; and the chief object of the author is to recommend the use of strips of adhesive plaster.

The following observations on cases of inflammatory diarrhœa please us, because they agree with observations which we had occasion to make some years ago on this subject, and which induced us to regard diarrhœa as the result of inflammation of the intestinal mucous membrane.

A short article, entitled, on Fracture of the Knee-pan, by Mr Robert Palk Mogridge, is communicated by Dr James Johnson. A longer and more elaborate one, on Fracture of the Knee-pan and of the Olecranon, by Mr Alcock, contains some observations sufficiently obvious, on the nature, effects, and treatment of these injuries, and many lamentations, and not a few examples, of the ignorance manifested in the detection and management of them. We presume that the unfortunate instances to which Mr Alcock alludes, are restricted very much, if not entirely, to the practice of his friends the apothecaries; for though it is well known that perfect osseous union of the knee-pan especially is difficult, if not impossible, yet we cannot conceive any person of moderate surgical attainments suffering the accident which Mr Alcock so pathetically describes to occur. We have ourselves treated these injuries, and seen them treated; but such results as he mentions or alludes to, we have not had the misfortune to witness. The Postscript of this paper is a most gratuitous and ridiculous effusion, whatever opinion of its merits the writer or his fellow associates may entertain. There is certainly in the profession,—a profession so miscellaneous and varied in its members,—great indifference and inattention, some ignorance, and a good deal of rashness. All of us know this, and deplore it; but the causes of it are as much out of our control, as the motions of the planets, and the rise and fall of the waters of the Nile, were out of the power of the astronomer of Cairo; and Mr Alcock is in a hallucination, not unlike that described by Imlac, when he fancies that his interesting monopoly of medical humanity and attention is either to effect the changes which he wishes, or to recommend himself and partners to the worship and patronage of the public.

The observations of Dr Armstrong, on the utility of opium in certain inflammatory disorders, are, like all the productions of that popular author, good and sensible, but neither new nor original;—at least we have often witnessed, and sometimes practised, the exhibition of the drug in the manner described. The paper should not have been in the Transactions of the Associated Apothecaries, for Dr Armstrong is not an ordinary member of this Association.

The 21st is a Case of Blighted Ovum, by M. J. Hayes.

The 22d, two Cases of Obstructed Colon, by Mr Callow, are too long to admit of our dwelling much on them. Both of them are interesting cases;—the last is instructive. It is difficult to say, however, whether the primary disease was in the colon or not, and whether all the morbid changes afterwards met with were not the result of the same general cause.

The 23d, an unusual case of Twin Conception and Labour, also a case of Blighted Ovum, with Remarks on the doctrine of Superfœtation, by Mr Powell, Surgeon and Accoucheur to the Lying-in Institution, Newman Street, London. The phenomena of blighted conceptions he believes inconsistent with the doctrine of superfœtation.

A case of Cut Throat, successfully treated, is good, and contains one or two useful remarks. In other respects it is not distinguished.

A case of poisoning by Opium by Mr Hayes, shows what all the world except the Association well knew,—that sulphate of zinc is the best emetic for expelling the narcotic from the stomach. Mr Hayes, we presume, is a young author, and wishes to show the profession, in this maiden effort, that he can write, speculate, and make remarks.

The remarks on *Neuralgia* and *Aphonia* by Dr Uwins, are smart, critical, and philosophical, but they have no right to a place in the present volume.

Mr Alcock's accomplishments are positively omnigenous; good reader! excuse this dreadful word, for we cannot, in our whole vocabulary, get another to express what we mean;—which is, that Mr A. excels in all things. We thought that we had taken leave of him some score of pages ago; but here he is again, talking of uniting fissured palate, and *reef-knots*, and the distinction between the reef-knot and the *granny's knot*. We learnt many summers ago, ere we knew what surgery was, the sailors knot, and the soldiers knot, and made a point always of tying every thing in surgical operation, from the knots of a pulley-rope, down to the ligature of an artery, with the sailors knot. Mr A.'s case was successful.

May we ask the worshipful Association, what this memoir of the professional character of Mr Morel has to do in its Transactions? In what manner does the knowledge of Mr M's virtues and talents contribute to the objects of the Association? We do not doubt that Mr Morel was a scholar, a gentleman, and though a pupil and lover of Pott, not insensible to the merits of John Hunter. But are not all so? and why do the Association come forward to tell the world this? Have they actually published this memoir for the purpose of venting their spleen at the present Director of the Army Medical Board, and awakening public compassion for the supposed injured Mr Morel? The Association certainly travelled most widely and gratuitously from their path, when they thought of giving publicity to a statement so grossly erroneous, and of uttering language so completely unfounded, as that contained in the note on the York Hospital. It is our duty to contradict every thing relating to the alleged unfairness of the treatment shown to Mr M., and we do so on official authority. We beg to inform the Association, that Mr Morel had served on full pay only 16 years, and though he had completed a service of 20 years, he could not have been entitled to full pay, nor to any thing beyond the usual half pay allowance. We further state, that in consequence of the reduction which took place in our military and medical establishment in 1817, it was requisite that all those who were retained, should be fit for service. Mr M., whatever might be his qualifications, was completely *ab agendo* long before he retired, or was obliged to retire, as the Association improperly state. The exchange which he made, if not in his favour, was at least attended with circumstances as gratifying in every respect, as it was possible in a case of the kind. In consequence of his impaired health, the heads of his department made strenuous efforts with the Secretary at War and the Board of Treasury, to obtain for him an annual pension. The application was unsuccessful; but Mr M. was grateful for the exertions which were made. On retirement he obtained the brevet rank of Deputy Inspector of Hospitals, as an honourable mark of approbation of his conduct as a medical officer.

Such are the contents accessory and essential of the first volume of the Transactions of the Associated Apothecaries of England and Wales; and if it be a fair average specimen of what is to be expected from their united labours, we do not think it will in any degree tend to raise their character in public or professional estimation. We have, indeed, been greatly disappointed. We did not expect a work equalling or rivalling the *Journal de Pharmacie*, or the periodical effusions of the various associations of apothecaries in Germany; as we well know that the apothecaries in England are not *pharmaciens*, nor possess the scientific devotion and pharmacological accomplishments of the Germans. But knowing them to be in possession of a certain range of practice, from which the higher classes of the profession are excluded, it was natural to expect some good plain observations of the common popular diseases, and the effects of remedies, with an occasional singular case when it happened not to have been examined by a more competent practitioner. It was also natural to expect, that among men familiar with the phenomena of chemical and pharmacological processes, some two or three individuals would appear, who could communicate new and useful information on the properties or physiological effects of some of the less known drugs and medicines. It is evident, however, that neither a Robiquet, a Pelletier, nor a Sertuerner is to be found among the Association. It is evident, also, that we were completely mistaken in our estimate either of the pretensions or of the capacity of the associated apothecaries. The present volume furnishes evidence that they aim at nothing short of becoming the most learned and scientific branch of the profession, before whom physicians and surgeons must cap, with a proper sense of their own inferiority; and that they are resolved to persuade the public that the most complete and extensive medical education is to be acquired, not as has hitherto been imagined, by attendance on lectures and hospitals, however sedulous and lengthened, but by five years of servitude in the mixing of draughts, and the preparation of pills and boluses. Whether they will succeed in this lofty aspiration, as people speak in these aspiring times, by any means, and whether they will succeed by the authority of Dr Armstrong, Dr Johnson, Dr Uwins, and other extra-association names, whom they so eagerly worship as the gods of their idolatry, time only will show. The Association of Apothecaries, if they wish to command respect, should stand on their own feet and be supported by their own strength, and not come forward to public notice, tottering in a new and unknown path, and encumbered with adventitious aid.

III.

Die gänzliche Extirpation der carcinomatösen Gebärmutter, ohne selbst enstandenen, oder künstlich bewirkten Vorfall, vorgenommen und glücklich vollführt; mit näherer Anleitung wie diese Operation gemacht werden kann. Von Dr Joh. Nep. Sauter, Grossherz. Baden, Medizinalrath, &c. &c. pp. 200. 8vo. Constanz, 1822.

The surgery of the present day is not more characterized by the science of its principles, than by the boldness of its practice. It is not only in the capitals of large kingdoms that the passing of ligatures around arterial trunks, second in importance only to the aorta, is practised as a routine operation, and the removal of a whole extremity is undertaken; but in the back settlements of America, surgeons venture to lay open the abdomen, and cut out from behind the intestines diseased ovaries; and here we have an account of a surgeon of Constance successfully extirpating the whole of a morbid uterus remaining *in situ*, unappalled by the protrusion of the intestines through the vagina.

Few diseases are more painful, or more hopeless, than cancer of the womb; but, until the present instance, the despair of the sufferer has not met with an artist bold enough to run all hazards in attempting to give relief. But Dr Sauter was unable to resist the urgent entreaties of Genoveva Waldorf, aged 50, the wife of a labourer, that he should attempt to relieve her sufferings, even if she should die under his knife; and although the operation had never been performed, yet the accidental approach to it, in some cases of *prolapsus uteri*, in some measure justified his procedure.

On the 28th January, 1822, at 2 *p. m.*, Dr Sauter proceeded to perform the operation, assisted by his son and Mr Distel a surgeon. The only cutting instrument used was a fine, narrow scalpel, with a thin broad handle and a short convex cutting edge. The patient was laid upon her back and her legs kept asunder by the assistants. The rectum and bladder were emptied in the first place. Dr Santer tried, by introducing the fore finger of the left hand curved to act as a hook through the disorganized *os uteri*, how far the *uterus* could be pulled down; but the fungous substance tore and bled, and the uterus did not move. The fore and middle fingers were now introduced under the *os pubis* into the space which the vagina forms around the uterus; the knife was next introduced between these fingers, and the vagina slowly divided to the uterus. The point of the finger was inserted, the incision and the circular division of the vagina gradually effected. Dr Sauter next attempted to separate the lateral connexions as high as possible, again with his fingers in the mouth of the womb, pulled the uterus as far down as possible, and tried to separate the cellular connexions, partly with the handle of the knife, partly with the fore finger of the right hand, but could not succeed on account of the narrowness of the space and the strength of the connexions, and during this attempt, a great part of the fungus on the anterior lip of the *os tincæ* separated. Dr Sauter next tried the forceps. He introduced one blade into the mouth of the womb, and the other over its anterior surface, and then pulled with some force, and attempted to separate between the uterus and bladder by the handle of the knife and finger within the peritoneum, but in vain. Half an hour was consumed in these painful and fruitless efforts, and Dr Sauter found himself obliged to change his mode of operation and desist from all attempts to peel the uterus out of the peritoneum; and he took the desperate (and, to himself, unaccountable) resolution of cutting the uterus, *in situ*, entirely out. He now put two fingers of the left hand into the vagina, and into the separation made between the bladder and uterus, carried the scalpel between these fingers to the place to be cut, endeavoured to lay hold of a portion of the cellular membrane with the bent fore finger, and thus cut portion by portion between the fingers, always keeping close to the uterus, until he got into the cavity of the abdomen. He next laid hold of the peritoneum with the hooked fore and middle fingers, and pulled it down, and divided it with the knife, portion by portion, close to the uterus on both sides, until the anterior and upper connexion, along with the peritoneum, was divided as far as the lateral connexion which lies higher. He now introduced the whole left hand into the vagina, laid hold of the lateral connexions with the fingers as before, and cut off from above, downwards as close to the uterus as possible, all connexion of the uterus with the ovaries, Fallopian tubes, ligaments, &c. He was now able to lay hold of the *fundus uteri* with four fingers, and attempted to invert it forwards; but the patient pressing strongly down as in labour pains, the intestines protruded into the vagina, over his hand, and he was obliged to desist. At a third attempt, by the aid of an assistant, who made strong pressure above the pubes, the uterus was at last inverted and brought down, with its fundus, forwards to the external labia, the intestines following. He was, however, now able easily to complete the division of the lateral connexions, and that with the rectum from above, downwards, and thus to terminate this dreadful operation. The intestines were now reduced into their natural situation, the patient kept in a perfectly horizontal position, and some clean dry lint introduced. The hemorrhagy was never so great as to threaten danger, and it was not necessary to secure any vessel. About a pound and a half of blood might have been lost in all. But, for the sake of precaution, wads of lint, wet with solution of alum, were stuffed into the vagina upon the dry lint already there.

The patient bore the operation wonderfully, and did not feel faint till it was over. She then complained of pain, especially in the epigastrium. The body was covered with a cold sweat; the pulse very small, scarcely to be felt, and diffusible stimuli were given in small doses. In three hours after the operation she recovered; the sweat became warm, and the pulse better. She now complained of burning heat in the vagina, but had no pain in the abdomen; and those in the pelvis, the consequence of the operation, soon ceased.

The uterus thus removed was, by the measurement of a figure said to represent it in its natural size, about 2½ inches long, and two broad, and the fungus protruding from its mouth still larger.

The detail of the convalescence need not be long. For some days vomiting recurred at intervals, chiefly during the night and after food. She was freer from pain the day after the operation than she had been for months. On the following day the intestines could not be reached by the finger through the vagina. On the ninth day after the operation, she sat up in bed; and on introducing the finger into the vagina, the peritoneum was felt contracted into a hollow cone. On the 13th she got out of bed for a little. Her convalescence was retarded by local exfoliation of the mucous membrane of the vagina, the effect of the application of the concentrated solution of alum, bowel complaints and incontinence of urine; but, on the 16th of March, in other respects she might be considered as cured. On the 9th of May she left the hospital, and took to her ordinary occupations and mode of life. On the 19th she began to complain of pulmonary affections, and she died on the 1st June.

The body was examined next day. The skin was very pale, and of a waxy whiteness; the left leg and thigh, and the *labium* of this side, œdematous; the abdomen not distended. By the finger in the vagina, the pelvis was found closed; and in the whole space there was no swelling or ulcer, but the posterior surface of the bladder was open. In the thorax, the lungs were found œdematous, and filled with greyish mucus, the heart natural. In the abdomen, every thing was in a good state, and in its natural situation; no effusion, or indication of former effusion. The omentum covered the intestines, as usual. The liver was in all respects natural, the gall-bladder empty. The spleen healthy, as well as the kidneys. The stomach pale, flaccid, pretty large, empty, and not distended with air. The intestines throughout natural, in their proper positions, no where inflamed, or showing traces of former inflammation; the small intestines quite empty; the ascending colon containing a considerable quantity of feces, of a soft consistence. The abdomen was entirely closed towards the pelvis; the peritoneum in this situation, as every where else, had its natural colour; the intestines could be freely lifted from it, except at one point in the middle of the covering of the pelvis, where the small intestine, resting upon it, adhered firmly by its external coat to a firm whitish-grey membranous mass, half a line in thickness, about the size of a sixpence (6 kreutzer), and was difficult to separate; but the separation was effected without opening the gut. Behind this, towards the rectum, there was a more extensive adhesion, about the size of a dollar, which could not be separated without opening the pelvis. All these intestines were perfectly sound, no where contracted, and their functions unimpaired. The rectum passed right into the pelvis, quite uninjured, and the division quite sharp upon it. The pelvis, examined from above downwards, was without growth or ulceration, and every where perfectly healed. The *ovaries* were found in their natural situation, but appeared rather small. The Fallopian tubes could be distinctly made out.

But besides the narrative of his own case, Dr Sauter, without pretending to give a monography of this affection, has subjoined various discourses connected with it. He has prefixed some historical notices concerning preceding excisions of portions of the uterus, commencing with the well known case related by Professor Wrisberg * of Gottingen, in which an ignorant midwife cut off with a common breadknife the prolapsed portions of an healthy uterus. Dr Bernhard in Granbunden, relates a similar instance † which happened in 1802. Mr Hunter of Dumbarton, in 1799, was the first who intentionally and knowingly ventured to perform what had succeeded in the hands of ignorant barbarians. ‡ He was imitated by Dr Joseph Clarke in 1805, but both of these practitioners included the inverted uterus in a ligature before venturing to cut it off. The late Professor Osiander of Gottingen, so far back as 1793, proposed in his lectures to cure scirrhous womb by excision; and in 1801, he actually put his proposition to the test of experience. At the request of this eminent teacher, we inserted in a former volume of this Journal,* an abstract of a communication which he read on the subject to the Royal Society of Gottingen in 1808. Professor Ruet of Vienna operated unsuccessfully in 1813.† Professor Langenbeck of Gottingen extirpated a prolapsed uterus ‡ with success. Lazzari removed a polypus from the uterus by ligatures, and claimed for Monteggia the merit of having proposed extirpation before Osiander. ¶ Dupuytren of Paris is said ‖ to have operated seven times successfully on the neck of the womb. Osiander's operation also gave occasion for many polemical writings in the German and Dutch journals, upon the possibility and practicability of what he asserted he had done, and the Imperial Josephine Academy of Vienna made it the subject of a series of prize questions in 1810; but it does not appear that there was any successful competition.

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* Gotting. gelehrte Anzeig. No. 81. 1787.

† Siebold's Lucina, 1st B. 3tis Ft.

‡ Duncan's Annals of Medicine. Vol. iv. p. 366. 1800.

Dr Sauter next states, and controverts the objections made to the practicability of extracting the uterus, on the theoretical grounds advanced chiefly by Zang, Charles Wenzel, and Elias von Siebold.

Appended to the history of the case in which he operated, he resumes the general consideration of the subject, and treats, first of the extirpation of the carcinomatous or scirrhous uterus, in the true sense of the word, without prolapsus, and details the steps of the operation, by which, as he thinks, after his experience, it may be performed most easily and successfully. He then concludes with some observations in reply to the prize questions proposed by the Josephine Academy, chiefly as to the doubts of the practicability or utility of the operation in any case; but it is unnecessary to enter upon them, as the case detailed by Dr Sauter proves that the total extirpation of the unprolapsed uterus has actually been performed, and, in so far as the operation was concerned, successfully. Lithographic views of the diseased uterus, of the size of nature, after its removal, both entire and split longitudinally, and of the few and simple instruments which he recommends, are given.

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* Edin. Med. and Surg. Journal. Vol. xii. p. 286. 1816.

† Salzburg. Med. Chir. Zeitung. 1813. 3to B. S. 188.

‡ Langenbeck Neue Bibliothek. der Chirurgia 1ten B. 3. St. p. 551.

¶ Journal of the Medico-Chirurgical Society of Parma. Vol. ix. No. 1.

‖ Bibliotheque Medicale 1815, *also* Dictionnaire des Sciences Medicales, *article* Femmes.

IV.

1. *Elements of Medical Jurisprudence.* By THEODRIC ROMEYN BECK, M.D., Professor of the Institutes of Medicine, and Lecturer on Medical Jurisprudence in the College of Physicians and Surgeons of the Western District of the State of New-York. In two volumes 8vo. Albany (United-States). 1823.
2. *The Principles of Forensic Medicine, systematically arranged and applied to British Practice.* By JOHN GORDON SMITH, M.D., Lecturer on Political Medicine. Second Edition. London. 1824.

WHEN we reviewed in our last Number the Toxicological part of Dr Paris and Mr Fonblanque's work on Medical Jurisprudence, we imagined we had bid adieu for an interval to systematic treatises on that subject; and, considering the ample share of attention we have of late bestowed upon it, we should have been willing to turn for a season to other topics. But at the present moment the science of Medical Jurisprudence appears to be exciting an interest throughout Britain, such as we have not witnessed on any former occasion; and it would ill become us, who have been among its earliest and most uniform well-wishers, to neglect any opportunity of furthering its progress towards that station among the medical sciences, which we have been long convinced it should occupy. Relying on these considerations, then, and on the powerful intrinsic claims which the works now before us possess on the attention of ourselves and the medical world generally, we shall venture to conduct our readers once more to the path along which we have already so often led them.

We undertake this task with the greater pleasure on the present occasion, that we shall have to throw aside, for the first time on medico-legal subjects, the character of the austere critic. It has been our misfortune to handle with some freedom almost all previous works on the subject. And we have been induced to handle them more freely than many may have thought was called for, because we apprehend, that all early works on Medical Jurisprudence, especially of the systematic kind, should be viewed with peculiar jealousy by every one interested in its progress. For, as it is now circumstanced,—languishing still in its infancy, and struggling against the supineness, indifference, and prejudices of those who ought to be its most zealous protectors,—no greater mischance could happen, than that systematic treatises on it should pass, without warning, into the hands of the public, which, however meritorious, are nevertheless not on a level with its state of advancement, and do not present an expanded view of its general spirit and practical applications.

At length, however, the English language may boast, that it is possessed of a general work on Medical Jurisprudence, which will not only stand comparison with the best of the kind that the continent has produced, but which may also be referred to by every medical jurist as a monument worthy of his science, and as a criterion by which he is willing that its interest and utility should be tried. Under the unassuming title of Elements of Medical Jurisprudence, Dr Beck has presented us with a comprehensive system, which embraces almost every valuable fact or doctrine relating to it. Each of its diversified departments has been investigated so minutely, that few cases can occur in practice, on which it will be necessary to seek elsewhere for farther information. At the same time, by studying succinctness, and shunning those verbose oratorical details with which other writers, and particularly those of France abound, he has succeeded in rendering his treatise comprehensive within a singularly moderate compass. We may securely assert, that a work on the subject is not to be found in any language, which displays so much patient and discriminating research, with so little of the mere ostentation of learning. The opinions expressed both on general principles and on the particular questions which have occurred in courts of law, are given clearly and judiciously. There are few occasions, even where the points at issue are difficult and obscure, on which persons of skill and experience will be disposed to differ materially with him. In addition to these remarks on the general merits of the book, we have only farther to add, that, in conformity with the laudable custom of the German writers, he has supplied, throughout the whole of it, a most copious list of references to authorities; the want of which in other English works must have been severely felt by every student.

Dr Beck commences in the usual way with an Introduction, giving a brief history of the progress of medical jurisprudence. Like most writers on the subject, he has assigned its origin to the publication of the Caroline Code by Charles V. of Germany, in 1532. This appears to have been the first code of laws by which the assistance of the medical practitioner was formally required in the determination of criminal cases; and before the period when it was framed no express work was published on any branch of legal medicine. The only part of the Introduction which requires particular notice here, is his account of the progress of medico-legal knowledge in the United States. The attention of the profession in that country was first directed to it by the exhortations of Dr Rush, delivered in an introductory lecture at the University of Pennsylvania in 1810. But these exhortations seem to have been of little avail; for at the period when the present work was published, the whole American literature on the subject comprehended an edition by Dr Cooper of several British tracts, and a few inaugural essays, and periodical memoirs. Professors and lecturers, however, have been appointed in several of the Colleges; but our author is silent as to their success; and on the whole he appears to judge correctly in believing, that the United States will be governed in their reception of this as of the other sciences, by the light in which it is viewed in Great Britain, and will not receive it cordially till it has become a more favourite object of study among us.

Our author professes to follow no method whatever in the general arrangement of the subjects of medical jurisprudence. After briefly noticing the attempts of Fodéré and Dr Smith to distribute them systematically, he says, he "has found a confusion to attend all these attempts at arrangement, which is probably insurmountable." And we confess we are almost inclined to agree with him; for all the methods of arrangement yet proposed are very far from being either elegant or convenient; and in following them, authors have done little more than has been done avowedly by Dr Beck,—they have merely written a series of unconnected Essays.

These Essays, then, are nineteen in number, and are thus entitled. 1. Of Feigned Diseases. 2. Of Disqualifying Diseases. 3. Of Impotence and Sterility. 4. Of Doubtful Sex. 5. Of Rape. 6. Of Pregnancy. 7. Of Delivery. 8. Of Infanticide. 9. Of Legitimacy. 10. Of Presumption of Survivorship. 11. Of Age and Identity. 12. Of Mental Alienation. 13. Of Persons found Dead. 14. Of Wounds on the Living Body. 15. Of Poisons generally. 16. and 17. Of Mineral Poisons. 18. Of Vegetable Poisons. 19. Of Animal Poisons. Of these subjects, the twelve first are discussed in the first volume; and that of Poisons occupies three-fourths of the second. He has omitted altogether one essential subject, on which we should have been happy to become acquainted with the ideas and opinions of so patient and accurate an inquirer, namely, Medical Evidence. We have stated on a former occasion, that we apprehend its nature is very little understood, either by lawyers or by physicians and surgeons; and we are likewise fully satisfied, that this ignorance of its nature is the chief cause why it still continues in so low a state, and is treated with so much contumely.

In the subsequent remarks we have to make, we shall endeavour to present the reader with as short and complete an analysis as is possible of his mode of investigating the foregoing subjects.

1. In the First Chapter, on Feigned Diseases, Dr Beck commences abruptly, by pointing out the general rules laid down by Zacchias for the detection of imposture; and then describes succinctly the various diseases which are known to be imitable, and the differences betwixt those which are real and those which are feigned. He follows the distribution of Metzger, dividing them into "*external* infirmities, real in one sense, but arising from factitious and culpable causes,—and *internal* ones, whose origin is uncertain." His descriptions are uniformly clear and comprehensive, and are well illustrated by short apposite cases and references to others. The diseases mentioned are, alteration of the pulse; altered state of the urine; hæmaturia; incontinence of urine; suppression of urine; maiming and deformity; dropsy, and tumours of various kinds; excretion of calculi and various foreign matters; ulcers; hæmoptysis; hæmatemesis; jaundice and cachexy; fever; pain; syncope and hysteria; diseases of the heart; apoplexy; paralysis; epilepsy; convulsions; catalepsy; nostalgia; near-sightedness; ophthalmia; blindness and deafness, with or without dumbness. This list is not altogether complete.——It appears that the rogues of the United States are not surpassed in knavish skill by any European race of impostors. We question whether the fasting woman of Tutbury, or even Victor Foy himself, had a more difficult character to sustain than the subject of the following singular story. "A very curious work was published at Newhaven in 1817, under the title of *The Mysterious Stranger, or Memoirs of Henry More Smith.* It purports to be written by the Sheriff of King's County, N. Brunswick; and I have repeatedly understood, that there is no doubt of the authenticity of all the material facts. The hero of the story was a most accomplished villain. While in the prison at Kingston, (N. B.), he began to spit blood, had a violent cough and fever, and gradually wasted away, so that those who visited him supposed that his death was rapidly approaching. This continued for a fortnight; and at length his weakness was so great, that he had to be lifted up in order to take medicine or nutriment. A turnkey unfortunately, however, left the door of the prison open for a few minutes, in order to warm a brick for his cold extremities. On his return, Smith had dis-

appeared. After many adventures and hair-breadth escapes, he is now a prisoner in the Newgate of Connecticut. He confessed that he pretended to raise blood by pounding a brick into powder, putting it in a small rag, and chewing it in his mouth. He contrived to vary his pulse by striking his elbows, and said he had taken the flesh off his body in ten days, by sucking a copper cent in his mouth all night, and swallowing the saliva!" (p. 13.)

2. The Chapter on Disqualifying Diseases is exceedingly short, as he has thrown out of it the only two diseases which demand any detail, and treated of them in the third chapter. He considers, *first*, the disqualifications in civil cases, confining his attention to those which exempt from service on juries or as witnesses; *secondly*, the disqualifications for military service, and on that head he adopts, with a few variations, the rules laid down by the French code, of which a full account is given in our 6th volume; *thirdly*, the rules for certificates, and the laws respecting them. He has omitted one of the most important and most neglected branches of the subject, namely, the infirmities which disqualify from effecting insurances on lives. A treatise on these disqualifications is a great desideratum in medical jurisprudence.

3. The third chapter is on Impotence and Sterility; a knowledge of which may be required in charges of rape, in cases of disputed paternity, and in pleas for nullity of marriage. After stating the Mosaic and civil law on the subject, and then that of France, England, and America, he follows the distribution of Foderé, and divides the causes of impotence in the male into *absolute*, *curable*, and *accidental*, or temporary. The most important of these three classes is the last, as the causes of that kind affect the individual during marriage, and may therefore be considered in cases of contested paternity. For most of the modern codes, although they hold the husband to be the father of every child conceived in wedlock; nevertheless, allow the proof of a physical impossibility of connection from accidental causes during a certain term, as evidence of illegitimacy. Dr Beck gives a list of the diseases which may be presumed to be incompatible with sexual intercourse; but we apprehend, that the names of several are given much too vaguely; and that with regard to the influence of some others, no medical man would be able to speak with that degree of confidence, which the law properly demands in such questions. The causes of impotence in the female are either curable or incurable. Of the latter kind, four varieties are mentioned,—obliteration or excessive thickening of the sexual organs, an aperture betwixt the vagina and rec-

tum, irreducible prolapsus of the uterus or vagina, and cancer of these parts. The rest are in general curable. The causes of sterility are some of them incurable, others curable; and of the former some are distinguishable during life, others not till after death. This chapter is closed with a very singular case of divorce on the plea of impotence, which was tried at New York in 1670.

4. The next dissertation, on Doubtful Sex, contains a good assortment of cases, chiefly taken from European periodicals. The author divides them into cases: 1. in which the sexual organs are mixed, but neither of them entire; 2. in which males have unusual formations of the urinary and generative organs; 3. in which females have unusual formations of the generative organs; and after adducing examples of each, and showing that such monstrosities have often existed to so great a degree as to cause ambiguity of sex, he observes, that instead of being actual hermaphrodites as was once universally believed, persons so formed are generally destitute of the powers of either sex. He then notices the singular laws which prevailed respecting them when hermaphroditism was believed to exist, and concludes with an account of the mode of examining and ascertaining their actual sex.

5. Next follows a luminous dissertation on Rape. It commences with a concise description of the signs of virginity, which is wound up with the following judicious opinion. "It is not to be denied, that many of them may be equivocal; but, notwithstanding, it is the duty of the medical examiner to notice them, and that *in connexion with one another.* It cannot be supposed, that all those we have mentioned as present during the chaste state can be wanting, without justifying a strong suspicion against the female." The author then passes to the consideration of the signs of defloration and rape. Having mentioned them in brief terms, he observes that they are liable to material fallacy, especially in those cases in which they are most distinct, and in which their presence is most essential to the constitution of the crime, namely in young children. For the female organs in children are liable to spontaneous inflammation (*Percival's Medical Ethics and Wood in Med. Chir. Trans.* 7.), and to catarrhal affections (*Capuron, Méd. Lég.*), whose consequences can scarcely be distinguished from the effects of violent intercourse. Hence these marks are only *corroborating* and not *certain* proofs of rape; and must be considered in conjunction with the age, strength and state of mind of the parties, with a comparison of the sexual organs, and with the circumstances under which the alleged crime was committed. He next gives a very

full account of the laws of different countries against the crime of rape, including an abridgment of the Jewish, Athenian and Roman statutes, those of Charlemagne, and Charles the Vth, the early Welsh and English laws, the present statute law of England, Scotland, France, and America, together with many detached but interesting remarks on those of other European and Asiatic states; and finally, some appropriate criticisms on the present vague state of the English law relative to the facts necessary for the constitution of the crime. In two of the United States, namely, in Pennsylvania and Illinois, in the former by a decision of the judge, in the latter by positive statute, emission is declared to be not essential; and Dr Beck is inclined to concur with the views there taken of the nature of the crime. The Dissertation is concluded by an inquiry into some medico-legal questions connected with the subject; namely, 1. Whether the presence of the venereal disease in the female is in favour or against her accusation; 2. Whether a female can be violated during sleep, without her knowledge; and, 3. Whether pregnancy ever follows rape. On these points the opinion of medical jurists is tolerably stable and uniform. There is a question, however, subordinate to the last, which has been considered more doubtful, and on which we should have been glad to have known the result of the author's researches; namely, whether pregnancy may follow defloration. An answer to this question would be desirable, on various other legal proceedings besides charges of rape.

6. The subject of Pregnancy may come under the cognizance of the English medical jurist under two circumstances; when a woman pleads it to stay execution, or when a widow is suspected to feign herself with child, in order to produce a supposititious heir, and the heir presumptive claims a writ *de ventre inspiciendo*. After noticing these and other particulars, Dr Beck proceeds to describe the signs of pregnancy, which he considers to be six in number, 1. enlargement of the abdomen; 2. a change in the state of the breasts; 3. suppression of the menses; 4. constitutional derangements, such as loss of appetite, nausea, &c.; 5. motion of the fœtus; and, 6. alteration in the state of the womb, recognizable by *the touch*. The consideration of these signs, he thinks, will never entitle the examiner in legal cases to go beyond an expression of probability in his opinion. The causes of deception he has enumerated are Moles, Hydatids, and Physometra: other causes have likewise given rise to deception, but he seems to think they could not deceive during a medico-legal examination, which must be necessarily executed with much wariness. He next gives a

collection of the best authenticated cases of supposed Superfœtation from Zacchias, Buffon, Moseley, Denman, Maton, and Foderé; and without giving a positive opinion on its possibility, he allows that most of these cases may be explained away on the principles laid down by the opposers of the doctrine, but that one related by Foderé, on the highly respectable authority of Desgranges, appears to be the strongest yet adduced in favour of it, and very difficult to reconcile with the opposite view. As an appendix to the present chapter, we are presented with a short investigation of the question, whether a woman can remain ignorant of her pregnancy till the time of labour? It is answered in the affirmative; but he maintains, that such ignorance is possible under two circumstances only—when the person is an idiot, or when she has conceived while in a state of stupor. This question our readers will find fully considered in our number for July last.

7. The work now increases in interest, as the author approaches its more important departments. He treats of Delivery, 1. as it regards the mother; and, 2. as it respects the child. Under the first division are considered the signs of delivery in the living and the dead body; the circumstances under which it may be concealed or pretended, and the mode of distinguishing actual delivery; and then some questions connected with parturition, such as the possibility of a woman being delivered without the consciousness of it, and the possibility of a newly born child perishing after delivery from the inability of the mother to relieve it. He very properly answers both questions in the affirmative, but is disposed to be less indulgent to females than most other medico-legal authors. The Second Part of the Chapter on Delivery, as it respects the child, treats first of the signs of its death before or during delivery; then of the signs of its maturity or immaturity; and lastly, of the state in which it must be born to enable it to inherit. The only part of this discussion we shall notice particularly, is that relating to its capability of inheriting. In the first place, he observes, it must be born *viable*, that is, at a period of pregnancy so advanced, that by the rule of nature it is capable of living; and he concurs with other authors in fixing on five months, or 150 days, as the shortest possible period. The singular case related by Dr Rodman of Paisley, in the 11th and 12th volumes of our Journal, and believed by its author to be an instance of a child surviving, though born within nineteen weeks after conception, he considers to have been a case of birth between the sixth and seventh month, on account of its weight and size. Secondly, it must actually have survived birth. The

proof of life, according to most of the older European statutes, was the fact of its having cried; and this criterion is still retained in Scottish law. In England the rule is vague; in France, the proof must be that of complete and perfect respiration! Thirdly, it must not be such a monster "as evidently to bear the resemblance of the brute creation."

Essay 8th, on Infanticide, is unquestionably one of the most elaborate, complete, and valuable treatises in the whole book. It is composed by the author's brother, Dr John Beck of New-York, and was originally published as an Inaugural Dissertation in 1817. As we dwelt very fully on the subject of Infanticide not long ago, we shall not attempt an analysis of Dr Beck's labours; but simply transcribe the following methodical table of contents prefixed to the chapter. "1. History of Infanticide among different nations, ancient and modern. 2. The Murder of the Fœtus in Utero, or criminal abortion, embracing an account of its proofs and causes. 3. The Murder of the Child after it is born, with an account of its various proofs and causes. 4. The Method of conducting Anatomical Examinations in cases of Infanticide. 5. The Prevention of Infanticide, including a History of Legislation on the subject, and an examination of the effects of foundling hospitals." It affords us much gratification to find, that in all the questions which may occur on trials for Infanticide and Criminal Abortion, the opinions of Dr Beck and his brother coincide exactly with those we expressed while reviewing the works of Lecieux, Capuron and Hutchinson,—with those indeed of every well informed medical jurist in Europe. We cannot understand why, with this coincidence of opinion staring them in the face, our English and Scottish courts have of late given so much credit to the doubts and difficulties of inferior and inexperienced professional witnesses. We beg to call Dr Beck's attention to one material oversight in the present treatise. In enumerating the causes of fallacy in the application of the hydrostatic test, he has omitted that very essential one, first pointed out by Chaussier, arising from the evolution of air in the cellular tissue of the lungs, independently of putrefaction. (*See the Number of this Journal for July* 1823.)

9. This chapter includes an investigation of the different medical topics which concern the subject of Legitimacy. It contains five short sections, on the ordinary term of gestation, on premature delivery, on protracted delivery, on the laws relative to the above subjects, and on some questions respecting paternity and affiliation. The author espouses the doctrine of the uniformity of the term of utero-gestation, believes it *barely* pos-

sible that a seven-months' child may occasionally be mature, and joins the ranks of the opponents of protracted pregnancy. Relative to the fallaciousness of the numerous reported cases of long protracted pregnancy, he adverts to a singular fact, not much attended to by the advocates of the doctrine; namely, that they have scarcely ever been witnessed in Britain or America, or any other country, in which the nature of the laws, or the looseness of their administration, did not afford encouragement to imposture. He farther observes, " that a calm and deliberate examination of these histories must certainly lead to a total disbelief of the doctrine of protracted gestation." We fear, however, that his calmest deliberation will occasionally be foiled in giving a rational account of some of them.

10. We do not believe that much information is to be procured from medical inquiries in questions relative to the Presumption of Survivorship. A great deal of refinement on the matter has been permitted in foreign countries. In American, as in English law, it is inadmissible. The only good purpose to be served by a physiological investigation of the subject, would be to establish feasible principles for the enactment of arbitrary statutes.

11. Questions of personal identity have been common on the Continent, and are not unknown even in our own country: But the aid of the physician will seldom be required to determine them. Dr Beck has added to the well known French cases, the following singular account of an error as to personal identity, which took place in 1804 at New-York. The case is also related, though somewhat differently, in the work of Dr Gordon Smith.

" An individual was indicted and tried at New-York in 1804, on a charge of bigamy, and the whole evidence turned on the question of his identity. He was called Thomas Hoag by the prosecutor, but stated himself to be Joseph Parker. Several witnesses swore that they had known him under the name of Thomas Hoag, among whom was a female, whom he had married, and afterwards deserted. It was stated, that Hoag had a scar on his forehead, a small mark on his neck, and that his speech was quick and lisping. All these peculiarities were found on the prisoner. Two witnesses deposed, that Hoag had a scar under his foot, occasioned by treading on a drawing-knife, and that this scar was easy to be seen, and had been seen by them. On examining his feet in open court, no scar was to be found on either of them; and it was farther proved, that at the time of the alleged courtship of the second wife in Westchester county, he was doing duty as a watchman in the city of New-York. The jury acquitted him." (I. 333.)

12. The author employs the term Mental Alienation in its

widest sense, to comprehend all unnatural states of the mind, which may become the subject of inquiry by the medical jurist. His Dissertation on this intricate subject possesses, in a high degree, all the characters which are so eminently displayed throughout the work in general. Of course it is quite unsusceptible of analysis. He treats first of the symptoms which constitute insanity, comprehending under that term the four species laid down by Esquirol, namely mania, melancholia, demency, and idiocy. The second section is occupied with the means of distinguishing feigned, and discovering concealed insanity. The third contains a commentary on the legal definition of a state of mental alienation, and the adjudications under it. In the fourth, he considers inferior degrees of diseased mind not included under the four idiopathic species already mentioned. One of the most remarkable varieties of this nature, is that produced by prolonged intoxication in habitual drunkards; a variety which seems to have become so frequent in some parts of the United States, that express enactments have been made to rank them on a level with lunatics. "In the State of New-York, we have a statute which places the property of habitual drunkards under the care of the Chancellor, in the same manner as that of lunatics. The overseers of the poor in each town, when they discover a person to be a habitual drunkard, may apply to the Chancellor for the exercise of his power and jurisdiction." This section might have been advantageously extended to a much greater length. Symptomatic mental alienation, occurring during the course of diseases of the body, present, in relation to medical jurisprudence, several interesting points of view, which have hitherto been almost entirely overlooked. The Fifth Section lays down the state of mind necessary to constitute a valid will. The observations made on this point are judicious; but they might have been arranged more logically under the head of the Third Section. Lastly, the Chapter finishes with an interesting litttle dissertation on the Deaf and Dumb—their capacity, and the morality of their actions. It has been decided by several cases, both in the English and Scottish Courts, that the deaf and dumb may obtain possession of their real estate, if they show sufficient understanding; that they are also competent as witnesses, if their understanding be strong, and their means of communication intelligible to those accustomed to their intercourse; and that they may farther be tried for crimes, if they are capable of distinguishing right from wrong, and are sensible that punishment follows the commission of crime. The following case has been added by Dr Beck to those already before the public.

" A deaf and dumb person was indicted for larceny in Massachussets; and being set to the bar for his arraignment, the solicitor-general suggested to the Court that he was deaf and dumb, but that the evidence would prove him of sufficient capacity to be a proper subject for criminal prosecution, and that he had formerly been convicted of larceny; and he moved, that one Nelson, then in Court, and an acquaintance of the prisoner, should be sworn to interpret the indictment to him, as it should be read by the clerk. The indictment was accordingly read by a sentence at a time; and Nelson having been sworn, explained its purport to him, making signs with his fingers. After which, the Court ordered the trial to proceed, as on a plea of not guilty." I. 384.

We apprehend that, in most cases of such a crime as larceny, our Courts will have little difficulty in deciding whether a deaf and dumb man should be put on his trial, whatever may be their difficulty with regard to such crimes as murder. The very nature of the crime goes far to establish the person's capability of dole, and power of discerning betwixt right and wrong.

Chapter 13th, the first of the second volume, and entitled, " Of Persons Found Dead," is appropriated to a description of the mode of examining a dead body, when death has been unexpected or unintelligible. The arrangement of the topics introduced into it is exceedingly arbitrary and confused; and many of the subjects are but imperfectly investigated. The author, after mentioning in a few words the nature of the coroner's office, announces his intention of considering the causes of sudden death, *independent of murder;* then treats, in two or three pages, of death from natural disease, from cold, and from lightning; next deviates to the art of medico-legal dissection; and, finally, discusses the subjects of death by noxious inhalations, by hanging, strangling, drowning, smothering, wounds, burning, and starvation. However difficult or impossible it may be to make a logical arrangement of all the topics of medical jurisprudence, it is not on that account to be inferred that the subordinate arrangement of its parts is to be neglected where it is obvious and natural. All the topics included in the second volume of our author's work, may be considered in a very natural and elegant order. After describing the mode of examining a dead body externally and internally, leaving out, for the time, the minute details of the signs of the different kinds of death, he might have gone on to relate the several causes of sudden death; 1. natural; 2. violent; and comprehended under the latter every variety, whether the result of accident or criminal intention.

It is to be wished, that, while mentioning the duties of the coroner, he had also laid down more precisely those of the medical inspector, under the coroner's jurisdiction. Some rules or instructions to this effect are loudly called for at present in England; for, in consequence of the murmurs which have been lately excited among the populace by the folly and carelessness of the anatomist's providers, the public antipathy has been turned even against licensed pathological examinations; so that coroners' juries, in their impertinent zeal, have frequently exerted their little brief authority for the purpose of checking them; and even in judicial cases have attempted to obstruct the medical examiner in the very act of discharging his duty. On a late occasion, one of these grave bodies declared they would apply to the governors of a London hospital to put a stop to *unnecessary* dissections; and in many cases they have told an inspecting surgeon to stop in the midst of his work, *because they themselves were satisfied*. We know not how the law stands as to this; but it certainly appears rational, that, when a surgeon's testimony is asked, he shall be allowed to collect it according to the rules of his own art, rather than the scientific notions of a few small shopkeepers; and therefore, till better authority is procured for his guidance, we should certainly advise a witness to refuse his testimony altogether, unless he is permitted to satisfy himself by the fullest possible examination.

Dr Beck's account of the mode of conducting a judicial inspection is short and incomplete; and he has made little use of some of the best and most recent authorities on the subject. The section on death by Hanging is also very inferior in accuracy and comprehensiveness to the other parts of the work; the physiology of the subject appears obscure and not altogether sound, and the description of the signs of hanging somewhat incorrect. In the course of his remarks on death by Strangling, he has investigated the famous case of Sir Edmund Godfrey, alleged to have been murdered by the pretended Popish conspirators in the reign of Charles II., and he adopts the opinion that Sir Edmund was murdered; but we apprehend without sufficient reason. The medical evidence, though voluminous and minute, if diligently and impartially sifted, will be found to bear on neither side of the question. We cannot conceive how Dr Beck should maintain, that all the testimony brought forward on the trial of the three printers for libelling the nation, by asserting that Sir Godfrey had committed suicide, "only proved the fact of strangulation more strongly." The proof of strangling is quite inconclusive. Dr Smith has likewise taken the same view of this case; and in giving an abstract of the moral evidence, seems to assume it as valid and uncontradicted. Dr Smith is surely not ignorant, that, whatever may have been the manner of Sir E. Godfrey's death, the story sworn to on the trial of the alleged murderers was afterwards rendered null by the condemnation for perjury of Titus Oates and the rest of his gang, who had been witnesses on the occasion. Dr Beck's account of death by Drowning is good, and the analysis of the legal cases connected with it admirable. To the collection of important trials from European sources he has added another of equal interest, which occurred in New York in 1800. The section on Death from Wounds is entirely occupied with the means of distinguishing suicide, homicide, and accident. Several cases are related with his usual perspicuity and conciseness; but he has not thrown any new light on the subject, or even exhausted the knowledge already possessed. The section on Death by Burning is likewise somewhat imperfect. He seems to agree with the opinion now entertained by most medical jurists in Europe, as to the possibility of the Spontaneous Combustion, or rather of the Preternatural Combustibility of the human body. The subject of death by Hunger is noticed but cursorily.

14. Of Wounds on the Living Body. Under this head the author first treats of wounds generally, according as they are *slight*, *dangerous*, or *mortal*; then of the circumstances which modify their nature, namely age, constitution, antecedent or coexisting disease, irregularities or carelessness on the part of the patient or his personal attendants, neglect or mismanagement on the part of his surgeon, and insalubrity of the atmosphere; next of the nature and properties of wounds of particular parts; and lastly, of the laws regarding mutilation.

The rest of the second volume is appropriated to the consideration of the extensive and important subject of Poisons. He first treats of Poisons generally, and investigates the individual poisons according to the natural history arrangement, namely, as they are derived from the mineral, vegetable or animal kingdoms. We see no reason for this return to the exploded classification of the last century, nor for his objection to the modern system, that "it is founded chiefly on a view of symptoms." We apprehend that, in reference to legal medicine, no other basis of arrangement can be so useful. We cannot give an adequate idea of the manner in which this division of the work has been handled, without such details as our present limits will not permit. It appears to us to be the ablest medico-legal treatise on Poisons, which has yet appeared in the English language. Every subject has received its due share of notice.

Perhaps the author might have exercised a little more discrimination in describing the tests for the chief mineral poisons. He has collected together all the tests which have at different times been proposed for each, describes every one with great minuteness, but does not state explicitly enough on which of them the medical jurist ought in all cases to rely. Many of those he mentions are quite unnecessary, others too coarse, some of them not altogether correct.

We shall close this analysis by a short notice of some substances he has added to the list of poisons, which are not to be found among those collected by Orfila. They are of the vegetable kingdom, are known as poisons chiefly in the United States, and are stated to be such on the authority of American writers.

Lobelia inflata. Two species of this genus, the L. syphilitica and L. longiflora, have been designated as poisonous by Orfila in the last edition of his Toxicology. The present species, a native of the United States, and known there by the name of Indian Tobacco, or Emetic Weed, seems to possess powerful acrid properties. A quack-doctor was tried not long ago in Massachussets for poisoning a man with it, who was his patient. Being ignorant of its dangerous properties he prescribed several powders daily for four or five days; and persisted in his treatment, though each dose caused violent and long-continued vomiting. "At last the patient lost his reason, and became convulsed, so that two men were required to hold him. To relieve which, the doctor forced down two more of his powders, and the patient, as was to be expected grew worse, and continued so till he expired." The quack-doctor was acquitted, from the want of sufficient evidence of malice prepense.

Tanacetum Vulgare.—Dr Beck quotes the authority of his countryman Dr Francis, in support of the poisonous nature of the oil of this plant, the common Tansy. Dr Francis met with an instance in which it proved fatal to a pregnant female. The quantity taken was half an ounce. The symptoms were those of pure acrid poisoning, and proved fatal in a few hours.

The *Gelseminum nitidum*, or yellow jessamine, would appear to be an active narcotic. A child in North Carolina died in a state of stupor, two hours after eating some of its flowers; and the whole plant possesses the same properties. Even its effluvia are said to have sometimes induced stupor.

Spigelia Marilandica.—This appears to be also an active narcotic, which has sometimes proved fatal, and has frequently produced vertigo, dimness of sight, delirium, staggering, and other symptoms of narcotism.

Kalmia Latifolia.—The decoction of this plant excites giddiness when taken even in small quantities; it is fatal to sheep and oxen; and the Delaware Indians are said to poison themselves with it.

In noticing the work of Dr Gordon Smith, which is also announced at the commencement of the present article, it is almost unnecessary for us to do more than express our hearty concurrence with the approbation with which it has been already received by the British public, and which has enabled him to publish a second edition, after the short interval of five years. It is purely a practical work, being offered, as the author professes, to the consideration of magistrates, coroners, barristers, and medical practitioners, to assist them in putting and answering such questions as may occur in judiciary investigations. The subjects included in it are therefore treated almost entirely with a direct view to their practical bearings; and he has deviated much less than most other writers into the physiological inquiries connected with them. Having originally intended to compose a Manual of Medical Jurisprudence, he has adhered faithfully to the most indispensable quality in such a work, namely, condensation. This object has been accomplished without any material sacrifice of its comprehensiveness; we believe the reader will find few investigations of note in the most voluminous authors on the science, which have not received a share of Dr Smith's regard. On some occasions, however, his dread of over-writing appears to us to have withheld him where he might reasonably have been more minute in his details. This is particularly apparent in various parts of the Chapter on Poisons; a subject which all general works on Medical Jurisprudence should, and commonly do, investigate more thoroughly than other subjects; because in them alone is contained almost all our information respecting Toxicology. Another defect, and not a trifling one in a Manual, is too sparing a reference to authorities. Where so many topics are adverted to but cursorily, it would have been very desirable had he referred the reader to sources of more extended information. In several places, too, we imagine he has sacrificed the clearness of his expositions to their conciseness. For example, in announcing the tests for the oxide of arsenic, he remarks: "With a simple aqueous infusion of arsenious acid, the following changes of appearance may be produced by various substances. If lime-water be added, it will cause a precipitate of a white colour; nitrate of silver, either in the solid form or in solution, will form a yellow sediment; sulphate of copper precipitates a green substance, &c." Dr Smith cannot be ignorant, (for he gives a more correct statement in the sequel), that neither nitrate of silver, nor sulphate of copper, will of themselves cause such precipitates in a solution of arsenious acid. Errors such as this, however, are by no means common. His opinions on the general doctrines of Forensic Medicine, and likewise on the sentiments of medical witnesses delivered in courts of law, appear to have been formed after a very full and masterly view of the subject, and are always expressed with great caution and accuracy. In short, as a treatise on the principles of the science, it may be uniformly followed with the utmost confidence.

The arrangement he has adopted is peculiarly his own, and is perhaps the best hitherto proposed. He forms four great divisions or classes of the subjects of Forensic Medicine. The *first* includes all inquiries relative to the extinction of life, and is divided into four sections—of Death in the healthy state, (a quaint expression, we presume, for sudden natural death)—of Death where there is question of Personal Agency—of Death by spontaneous Personal Agency—of *Prolicide*, including *Fœticide*, or Criminal Abortion, and Infanticide. The *second* division treats of those questions which arise from injuries to the person, not leading to the extinction of life; and under this head are considered the medico-legal illustrations of the crime of maiming—of corporal punishment—and rape, together with the legality of surgical operations. Under the *third* division, he investigates the subject of Disqualifications, 1*st*, of the Mind, 2*dly*, of the Body, and, 3*dly*, pretended disqualifications. The *last* great division embraces a few Miscellaneous subjects, which cannot be distributed philosophically throughout the other three—namely, the phenomena, consequences, and duration of Pregnancy, sexual Ambiguity, personal Identity, Survivorship, Insurances on lives, and medical Evidence. We recommend the last section, in particular, to the careful perusal of every practitioner. It contains some good views of the nature of medical evidence, and several judicious instructions for the conduct of witnesses.

V.

Intorno la Scoperta di Due Nervi dell' Occhio Umano Ragguaglio. Del Dr GIUSEPPE TRASMONDI, Professore di Anatotomia Pratica nel ven. Ospedale della Consolazione, all Eccellentissima Deputazione degli Ospedali di Roma. Estratto dal Giornale Arcadico, T. XIX. P. 1. Roma, 1823.

Notice of the Discovery of Two Nerves of the Human Eye. By Dr Giuseppe Trasmondi, Professor of Practical Anatomy in the Hospital of Consolation, &c. &c. From the Arcadian Journal. Rome, 1823.

The discovery of a new muscle, and of two nerves in the human orbit, is an event which, we believe, all our anatomical and surgical readers will hear with curiosity and interest; and we hasten to communicate, however hurriedly, the principal circumstances of its history, which it is desirable to know.

In the course of June 1823, it was stated in a Neapolitan Journal, entitled the " Medical Observer, " that Dr Hermer of Philadelphia had discovered a new muscle attached to the lachrymal bone of the human orbit. Dr Joseph Trasmondi, professor of practical anatomy at the Hospital of Consolation in Rome, having read this notice, determined immediately to take means to verify the existence of the new muscle; and on the 3d of July, after proper examination, recognised the muscle, though in the person of a woman of 70, and was enabled to ascertain its situation, figure, attachments, and the direction of its fibres; and on subsequent examination, he discovered two nervous twigs with which it is supplied, and which had not been noticed by previous anatomists. Of these discoveries he immediately presented an account, with several physiological and pathological observations, to the Deputation of Hospitals of Rome, by whom it appears to have been most graciously received.

The muscle of Hermer is said by Professor Trasmondi to be situate at the inner part of the orbit, between the posterior margin of the lachrymal bone (*os unguis*) and the lachrymal orifices. It rises by an aponeurotic line, which adheres to the bone in a vertical direction,—beginning at its upper angle, and terminating about four lines below. From this it proceeds forward with parallel fibres, passing under the corresponding portion of the lachrymal sac. Having reached the point where the lachrymal ducts unite, it divides into two equal portions, which accurately cover these ducts. In consequence of this disposition, it has a body and two extremities, the former of which is directed from behind forwards, while the latter are slightly arched, to be adapted to the situation of the lachrymal ducts. Its fibres are enveloped through its whole extent by very delicate cellular tissue. Its external surface adheres in the body to the lachrymal bone and part of the sac, and in the extremities to the lachrymal ducts. Its internal surface, which is covered by a thick layer of fat, is at the extremity of the *conjunctiva*. In the angle formed by the two extremities, the semilunar membrane and lachrymal *caruncula* are lodged.

Professor Trasmondi having thus verified the existence of the new muscle of Dr Hermer, proceeded to inquire into the nerves by which it might be supposed to be supplied; and by patient and repeated researches, ascertained that it received two nervous twigs proceeding from the external nasal branch, which is derived from the ophthalmic division of the tergeminal nerve, or 5th pair of the earlier anatomists. After showing that this ophthalmic division sends, 1st, a branch to the lachrymal gland; 2d, a frontal; and, 3d, a nasal branch; and that Bichat, in his minute and accurate enumeration of their subsequent distribution, gives no proof either that he was aware of the muscle of Hermer, or any nerves distributed in that situation,—he proceeds to say, that according to his researches, the continuation of the external nasal nerve is not single, but about six lines from its first division, a second is found forming two ramifications. These minute branches or twigs run in the cellular tissue, one below, the other close to the trochlear muscle (*obliquus superior*), and are continued to the trochlear tendon. From this point the upper, after anastomosing with a filament of the frontal branch, and giving off all the minute twigs of the eyelids, nose, &c. bends inward. Here being united with the lower twig, which also bends inwards, they pursue their course together, during which they are again separated, and are finally implanted into the muscle of Hermer. Adhering to this by cellular tissue, they run along its extremities to the lachrymal orifices (*puncta lachrymalia*), in such a manner, that the twig which passes below the trochlear muscle goes to the upper extremity, while the other goes to the lower extremity.

We have given here the author's description of this muscle and its two nerves, that every one who has inclination or opportunity may examine them personally, and confirm or correct the discovery of Dr Hermer. We are not aware that any British anatomist has yet directed his attention to it, or attempted to verify the existence of the parts now alluded to. This, however, may arise from the researches of Dr Hermer and Professor Trasmondi being hitherto very imperfectly known in this country. The parts are represented distinctly enough in a neat engraving, which may serve as a guide to the investigation of the dissector.

It was not to be expected that a muscle and two nervous twigs should be discovered without inquiring into the purpose which they were destined to fulfil; and we accordingly find, that the greater part of the Memoir before us is occupied in speculations on the uses and functions of the newly known parts. We are informed that Hermer, the first observer of the muscle, ascribed to it the office of shutting the eyelids, and of rolling the lachrymal orifices towards the nose. It appears, however, that Professor Trasmondi, after sundry express discussions with Signor Joseph Mori, his zealous assistant in the necessary researches, could not agree on this use, and was induced, by its situation, its figure, its nerves, and its division, to ascribe to it powers which he regards as much more important and interesting.

He begins with remarking, that all the explanations hitherto given of the manner in which the tears are absorbed by the lachrymal orifices, and conveyed by the ducts into the sac, have been ever liable to great doubt. This, however, he thinks is at once removed by knowing the existence and situation of the muscle of Hermer. As its body covers the lachrymal sac, and its extremities the ducts, it is natural to think that its motions will have considerable influence over the capacity or situation of these minute cavities. The fibres attached to the ducts will lengthen them, and turn their extremities to absorb the tears, dilate them to admit this fluid, shorten and contract them successively to impel it along their narrow canal, and finally raise them so as to discharge it conveniently into the lachrymal sac. After continuing in this cavity for some time, they are expelled into the nasal canal by the contraction of the body of the muscle, which, as was shown, covers the upper part of the sac.

The orbicular muscle of the eyelids covers with some of its fibres, and its so named tendon, that part of the lachrymal sac which is not under the influence of Hermer's muscle. But it would be inadequate to effect the transit of the tears, unless the individual were to shut the eyelids every moment; and its circular direction is inconsistent with its furnishing any fibres to the ducts, which require a muscle that should exactly cross the orbicular one, in order that the eyelids may perform their functions without restraint. There is no doubt, Professor Trasmondi remarks, that when the tears are copious, the orbicular muscle facilitates their conveyance into the nasal canal; but it is well known, that in such a case the eyelids must be forcibly shut; that is, the orbicular muscle must contract to its utmost. It is further known, that by the elevation of the upper eyelid, the upper lachrymal duct is enabled to transmit the tears into the sac; but, besides that this elevation does not favour the action of the lower duct, the incessant motion would be inconvenient; and this inconvenience is obviated by the actions of the lachrymal muscle.

Professor Trasmondi further remarks,—that as the lachrymal secretion is a continued action, not only is the occasional or periodical motion of the orbicular muscle insufficient for the purpose of absorption and transmission—but an agent of uninterrupted powers is requisite; and this he conceives to be found in the muscle now discovered, which he regards as possessing a constant action *(un azione perenne)*, and consequently independent of the exercise of the will, and thus effecting the absorption, transmission and deposition of tears at all times, in the waking or sleeping condition, without any power in the individual to suspend, diminish, or accelerate the function.

The discovery of these nerves, according to our author, furnishes an explanation of several phenomena which occur in the secretion and absorption of tears. The same nerve, viz. the ophthalmic branch of the 5th pair or tergeminal, furnishes a twig to the lachrymal gland, a twig to the cavity of the nostrils, and two twigs to the new muscle. This disposition shows that these parts communicate mutually by means of nervous branches. If, for example, in consequence of the irritation of any one agent, the secretion of the lachrymal gland is increased,—the action of the new (lachrymal) muscle is at the same moment augmented, in consequence of which the lachrymal orifices, ducts and sac, receive the separated fluid, and discharge it into the nasal canal. In like manner, if the irritation of any other agent in the cavity of the nostrils is followed by an equal secretion of tears, they are absorbed and conveyed away by the same means.

The only other point in this physiological inquiry to be explained, is the reason why the lachrymal fluid, when deposited in the sac, continues there for some time, before it passes into the nasal canal. Physiologists have generally supposed, says our author, a sphincter muscle, to which they have ascribed the power of preventing the constant passage of tears into the nostrils. Professor Trasmondi, however, regards this phenomenon as the result of the column of air which is incessantly introduced into the nasal canal by means of respiration, and regards this imaginary sphincter as unnecessary.

It is undoubted, he observes, that the nasal canal being a cavity, situate in the anterior part of the corresponding nostril, admits a portion of the air introduced into the nostril by respiration; and it is also undoubted, that this air ascends through the canal during inspiration, but descends during expiration. In proof of this, he adduces the fact,—that in lachrymal fistula, when the obstruction of the nasal duct is removed, if the patient blow the nose, not only purulent fluid but also air is discharged by the fistulous opening in the inner angle of the eye. He further observes, that a column of air will sustain a fluid more dense than itself, providing this fluid form a column less heavy than its own. To estimate the proportion subsisting between the air introduced into the nasal canal, and the tears discharged during the same time from the lachrymal ducts into the sac, it is requisite to observe the proportion between the capacity or *calibre* of the nasal canal, and that of the lachrymal ducts; and this proportion he states to be about eight to one. If these principles be admitted, it cannot be denied that the fluid discharged, tear by tear, from the ducts into the sac, will be sustained by the column of air introduced into the nasal canal, and that it will be unable to overcome this resistance, until such a quantity be accumulated as to exceed the pressure of the air, when the contraction of the body of Hermer's, or the lachrymal muscle, will force it toward the lower part of the lachrymal sac. By this arrangement, the author also explains the existence of the different angles mutually formed by the lachrymal passages, and their several capacities; for if these recipients, says he, were united, and formed a single straight tube, of a conical shape, with its basis at the lower opening of the nasal canal, and its apex at the lachrymal orifices, the occasional motion of the nasal column of air would cause the expulsion of the tears when they were scarcely introduced, and would retard or resist their transmission to the nose. These speculations and reasonings are, it must be avowed, very neat and ingenious, and explain the phenomena with much elegance. We trust that, ere long, the researches of our anatomical observers will give them that certainty which every lover of correct science must wish.

The influence of these newly known parts in the diseases of the eye and its appendages, gives rise to some further disquisition. He regards the lachrymal muscle as liable to palsy and spasmodic contraction, either of which affections may contribute to impede the transit of the tears, and give rise to epiphora, or lachrymal fistula. Its body may be paralyzed, while its extremities retain their power; as happens in those persons in whom the sac is filled with unaltered tears, especially during sleep, and which are expelled by simple pressure of the finger through the corresponding nostril,—showing that there is no organic or other obstruction of that conduit. Professor Trasmondi adduces a variety of observations, to show the practical importance of this fact; and to prove that fistula, or other disorder arising from this palsy of the lachrymal muscle, cannot be cured unless the action of the muscle be restored. This palsy, indeed, he regards as a frequent, but little suspected, cause of relapse in this disease.

He therefore, under these circumstances, delivers three conclusions of practical importance. *1st*, In epiphora arising from mere obstruction of the lachrymal orifices, the obstruction should be freely removed by means of a small probe, with a certainty of success, since the muscle is not affected, and the lachrymal orifices and ducts will continue their action. This method has been hitherto regarded as useless, while the orifices and ducts were supposed to possess the power of attracting the tears, and to lose this power when obstructed. *2d*, In epiphora caused by palsy or spasmodic contraction of the lachrymal muscle, as its nervous filaments are derived from the ophthalmic branch of the tergeminal pair, suitable remedies should be applied beyond the inner angle of the eye, where the muscle is seated, at the root of the nose, upon the eyebrows, in the corresponding nostril, on the frontal muscle, and, in short, on the malar bone of the affected side,—parts in which the productions of the ophthalmic nerve are found. *3d*, The development of lachrymal fistula may be arrested in the beginning, when it is known that the tears are retained in the sac by palsy only of the lachrymal muscle. *4th*, Finally, it is possible to prevent relapse of fistulous disease, by administering, after the cure, remedies applied to the muscle and nerves, so as to rouse those vital forces which have been too much diminished in the course of the disease.

These are the most important contents of this little communication. Of their value, their interest, or their importance, we offer no opinion. Every one will think differently, according to the nature of his own pursuits, and according to the peculiar light in which he is accustomed to view those of others. We can only say for ourselves, that we have been much gratified, not only with the information given by Professor Trasmondi, but by the perspicuous and elegant manner in which it is communicated.

VI.

1. *De Præcipuis Acidi Prussici et Aquæ Cohobatæ Laurocerasi Medicis Facultatibus Clinicis Observationibus comprobatis Specimen.* J. A. Manzoni. Patavii, 1818.
2. *Recherches et Considérations Médicales sur L'Acide Hydrocyanique, son Radical, ses Composés et ses Antidotes.* Par Jph.

COULLON, Docteur-Médecin de la Faculté de Paris, Membre de plusieurs Sociétés. A Paris, 1819.

3. *Recherches Physiologiques et Cliniques, sur l'Emploi de l'Acide Prussique ou Hydrocyanique dans le Traitement des Maladies de Poitrine.* Par F. MAGENDIE, D. M. Paris, 1819.
4. *An Historical and Practical Treatise on the Internal Use of the Hydrocyanic (Prussic) Acid in Pulmonary Consumption, and other Diseases of the Chest, as well as in several Complaints, &c. &c. &c. Illustrated by numerous Cases.* By A. B. GRANVILLE, M.D. F.R.S. F.L.S. M.R.I. Physician in Ordinary to H.R.H. the Duke of Clarence. London, 1820.
5. *C. W. Hufeland, Vorschlag, Statt der Blausäure das destillirte Wasser der bittern Mandeln zum medizinischen Gebrauch anzuwenden, &c. &c.* Berlin, 1822.
6. *Observations on the Remarkable Effects of Iodine in Bronchocele and Scrophula. Being a Translation of Three Memoirs published by I. R. COINDET, M.D. of Geneva.* London, 1821.
7. *Saggio Clinico Sull' Iodio e sulle differenti sue Combinazioni Farmaceutiche giusta i risultamenti che se ne sono ottenuti nell'istituto Clinico-Medico dell' I. R. Università di Padova. Estratto dal Prospetto Clinico dell' Anno Scolastico* 1820–1821. Padova, 1822.
8. *Essay on the Effects of Iodine on the Human Constitution; with Practical Observations on its Use in the Cure of Bronchocele, Scrofula, and the Tuberculous Diseases of the Chest and Abdomen.* By W. GAIRDNER, M. D. London, 1824.

AMONG the new remedies which of late years have been gradually getting possession of professional confidence, and may be now said to be added to the pharmacological lists of the practitioner, hydrocyanic acid, and iodine and its preparations, hold a very distinguished place. Their decided characters as physiological agents, and their alleged effects in the cure of diseases, call on us, even at this late period, to bestow a short and hasty notice on their virtues, and their employment in the practice of physic.

It was in the year 1780 that Scheele first taught the chemical world, that the substance termed *prussian blue*, which had then been known for seventy years, consisted of iron combined with a peculiar acid, which was afterwards named *prussic* by Guyton-Morveau. The subsequent researches of the elder Berthollet and of Fourcroy, showed that this acid consisted of carbon, hydrogen, and nitrogen. Porrett attempted to assign the proportions of each, * and inferred the existence of another acid, which he termed the *ferro-chyazic ;* but it is to Gay-Lussac that we are indebted for the knowledge—that the prussic vapour, if deprived of its hydrogen, forms a gaseous substance which is the basis of the prussic acid, and to which, for that reason, he applied the denomination of *cyanogen.* * The acid liquor which is formed by the union of hydrogen with this substance, he termed the *hydrocyanic acid*, and that arising from the union of equal portions of chlorine and cyanogen the *chlorocyanic acid.*

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* Carbon, 24.8; Hydrogen, 34.5; Nitrogen, 40.7.

If we admit, as is ascertained by chemical analysis, that the active principle of bitter almonds, the leaves of the cherry-tree laurel, the peach blossom, and other similar vegetable substances, is hydrocyanic acid, then it may be said that the use of this substance in medicine is very ancient. The cohobated liquor of the black cherry was an officinal preparation in the pharmacopœias of the London and Edinburgh Colleges, till the experiments of certain physicians in Worcester, mentioned by Lewis, †, demonstrated its fatal effects. The distilled water of the cherry-tree laurel was used in certain disorders of the stomach by Duhamel du Monceau, and Browne Langrish; Baylies was in the habit of using it in the treatment of rheumatism and asthma; Cameron considered it very efficacious in obstructions and obstinate diseases of the liver; Cheston recommended it in cancer of the face; and Perceval ascribed to it a resolvent power.

Hydrocyanic acid, however, though found in various parts of plants, does not exist in a state of purity in any natural production; nor can it be obtained of uniform strength for the purpose of medical administration; and although it did exist in the substances to which we have now alluded, the ancient use of these substances in the treatment of disease, would not furnish much accurate information on the use of the agent, as it is obtained from the laboratory of the pharmacologist. We learn from Manzoni and Dr Granville, that hydrocyanic acid was not employed in this form before the beginning of the present century; and that previous to 1806, Borda, Brugnatelli, and Rasori administered it in diseases which they considered as distinguished by high excitement. Brugnatelli had stated in his Farmacopeia of 1802, that it was useful in pneumonia, angina, tubercular cough, catarrh, &c.; and we learn from Manzoni, that Professor Brera of Padua, who had previously (1797) used the cyanuret of mercury in syphilitic affections, was in the habit, after 1809, of administering hydrocyanic acid in pneumonia and other diseases of the lungs, in the Clinical Institute of Padua. These examples seem to have rendered the practice pretty general in the northern states of Italy, and in some parts of Germany; for Sprengel recommended it not only in affections of the lungs, but in complaints of the stomach, hypochondriasis, &c.; and Hufeland employed it frequently, and successfully, in the treatment of various obstinate disorders, in 1812 and 1814.

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* Generator of blue.

† The New Dispensatory, &c. 3d Edition. London, 1770, p. 376.

In 1815, Dr Granville first called the attention of the profession to this medicine in England, in his observations on the use of hydrocyanic acid in pulmonary complaints. Professor Brera, in the following year, communicated the result of his experiments, in his Reports of the results obtained in the Clinical Institute of the University of Padua; and Magendie, who had tried the remedy in 1815, published his researches on its effects in 1817, and recommended it strongly as a remedy in consumptive disorders. The more complete researches of this physician, the elaborate memoir of Coullon, the thesis of Manzoni, the short account of Dr Elliotson, and the present edition of Dr Granville's work, with one or two others of less consequence, are the publications which have since appeared, and which have contributed to add to our knowledge of the effects of this medicine, or increase our confidence in its powers.

Dr Granville gives formulæ for the preparation of the hydrocyanic acid, according to the four processes of Scheele, Vauquelin, Magendie, Gay-Lussac, and the London Apothecaries' Company. So far, however, as the practice of physic is concerned, it is quite unnecessary to attend to any other but those of Scheele and Gay-Lussac, or to enumerate any other forms of the medicine than those obtained by these two processes. From the experience of Magendie there is reason to believe, that the acid obtained, according to the procedure of Scheele, is neither sufficiently uniform in strength, nor sufficiently pure, to be quite manageable. The truth of this we have in some degree the means of confirming by personal observation. In the summer of 1819, we had occasion to administer hydrocyanic acid in some affections of the chest, in which the account of Dr Granville led us to think it might be useful. None of the medicine, however, could be obtained in Edinburgh, and we had it prepared according to the process of Scheele, under our own immediate inspection, by a careful and intelligent friend, well versed in the practical details of chemistry. The hydrocyanic acid thus obtained, was found capable of killing small animals; and when exhibited in the human subject, it produced its proper and usual effects. It was found, however, not to act with uniformity; and while, in some persons, its ordinary doses were followed by evident sedative results, in others no change could be recognised. Magendie, we are informed, encountered the same inconvenience; and he has, therefore, been led to relinquish the acid of Scheele entirely, and substitute that obtained by the process of Gay-Lussac. The density of the acid procured in this manner, is at 45° to that of water, as 0.70,583 to 1; * but as this is greatly too concentrated for medical use, Magendie dilutes it with 6 times its volume or 8½ times its weight of distilled water; so that the weight of this preparation will be equal to 0.920583.

This hydrocyanic acid has been recommended strongly in various forms of consumption, in pulmonary hemorrhage and inflammation, and in certain forms of stomach complaint. Perhaps the greatest fault which could be charged against its patrons is, that it is too indiscriminately, and too generally commended as a remedy; and it may be feared that it will thus share the fate of many other agents, which, though possessed of some real virtues, are thrown aside contemptuously, because they do not possess all, and because they are not efficacious in the most opposite morbid affections. In the work of Dr Granville, for example, which is certainly the most elaborate account hitherto given of its medical properties and its employment in disease, it is recommended not only in the ordinary pulmonary affections in which it has been believed on just grounds to be remedial, but in tubercular disorganization, in chronic peripneumony and pulmonary abscess, in abortion, disordered menstruation; in nervous diseases, as hydrophobia, angina pectoris, epilepsy, and spasmodic colic; and in some organic diseases. Now, it is scarcely in the nature of things that any single remedy could control morbid processes so numerous, and in some instances so opposite; and certainly impossible to suppose that any remedy could remove pulmonary tubercles, or scirrhus of the stomach, or those opposite organic changes on which the symptoms termed *angina*, depend. Dr Granville has, indeed, by subdivision of diseases into varieties, attempted to distinguish those in which hydrocyanic acid is to be administered; but it may be doubted whether his views on this subject are quite correct. His first species of consumption is evidently that form of pulmonary disease called chronic pleurisy; and we are not aware that hydrocyanic acid has been known to exercise any favourable effect over it: We should doubt how far any species of consumptive disease could be said to be owing to a vitiated state of the animal fluids, which is the second of Dr Granville; and should doubt much more whether hydrocyanic acid could remove this condition. We cannot agree with Dr Granville at all in prescribing it in tubercular consumption; for never, in any case in which it could be ascertained that the lungs were tuberculated, have we seen it do good, and in some its exhibition has appeared to aggravate the sufferings of the patient.

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* The statement given by Dr Granville, though numerically correct, is not according to the usual fashion of computation. He makes it 70,583 to 1, by omitting the cypher; a thing of some consequence in decimal enumeration; and this in fact gives hydrocyanic acid the impossible quality of being 70 times heavier than water.

The most favourable forms of consumptive disease for the exhibition of this medicine, appear to be those depending on chronic bronchial inflammation, and that to which the German writers have applied the name of *florid phthisis.* In the former, it has been found to abate the membranous inflammation, reduce the quickness of the pulse, and relieve the cough; and, in one or two examples in which the symptoms, such as occasional hemoptysis, were to be referred to the latter, its exhibition has been known to be attended with disappearance of the complaints. Considerations of this nature appear to suggest its use in the third species of consumption enumerated by Dr Granville. On the propriety of employing it in that variety, if it be a variety, which attacks the female constitution during pregnancy, after parturition, or after long suckling, it is not easy to lay down very general rules. Dr Granville adduces nine cases, in seven of which the hydrocyanic acid was followed by restoration to health, while in two, the sixth and seventh, it was attended with no benefit, further than general alleviation of their sufferings. It may be remarked, that all cases of consumption which take place in the persons of females under the circumstances now mentioned, do not depend on the same morbid state of the lungs; for the same remote cause will often be attended in different individuals with different effects; and though many females are cut off after parturition with cough, expectoration, and other symptoms indicating catarrhal disease, yet not a few die without any appearance of this kind. The distinction, so far as the exhibition of hydrocyanic acid is concerned, is of little practical use.

Dr Granville strenuously advocates the use of hydrocyanic acid as an auxiliary to blood-letting in pneumonic inflammation. In this he is supported by the authority of Brera, who, in his report, repeatedly dwells on the advantages resulting from its use under such circumstances. * Its operation is quite similar to that of foxglove; and where it can be depended on as to its composition and actual powers, it may be usefully employed in cases in which blood-letting has been carried so far, as to render the propriety of its further employment doubtful. On this head we refer to Dr Granville's third section, p. 248.

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* Prospetto Primo, sezime II. p. 30.

In other inflammatory diseases, its advocates recommend hydrocyanic acid with not less confidence. Brera used it successfully, we are informed, in the treatment of inflammation of the spinal chord (*rachialgitis*), for which he is praised by his pupil Manzoni; and Dr Granville thinks he prevented the fatal termination of a case of inflammation of the diaphragm, and one of inflammation of the womb; but in both instances copious blood-letting had been previously practised, and the force of the disease was certainly broken. The hydrocyanic acid, in such instances, appears to operate very like opium, or any other sedative medicine, which, when the powers of the circulation have been reduced to extreme weakness, gradually induces a state of the system, during which the contractions of the heart become less frequent, but fuller; and the capillaries of exhalation and secretion at length resume their usual functions.

Asthma, or any other spasmodic difficulty of respiration, and the fits of hooping-cough, are relieved by this medicine, as they are by many others; but it will depend much on the result of other remedies, and on the peculiar views of the practitioner, whether he would employ an agent which requires very sedulous attention in watching its effects.

There is something paradoxical in the use of hydrocyanic acid in treating affections of the stomach. Every one is familiar with the unpleasant effects of eating the kernels of the bitter almond, ratafia biscuit, or any thing flavoured with this substance; and there are few indeed in whom a small quantity of noyau, cherry-water (*kirschen-wasser*), or any similar substance, would not suspend the action of the stomach much more effectually, and much more speedily, than an equal quantity of pure spirit of wine. The same, it might be argued, may be said of opium, or other narcotic substances, from which dyspeptics sometimes derive benefit. To correct, to modify, or to enlarge our ideas on the effects of hydrocyanic acid introduced into the stomach in this manner, we are not much aided by the elaborate researches of M. Coullon; but if we trust his observations in the only analogous case, that of the slow and lengthened action of the acid, it is very difficult to derive any confirmation of its good effects. "When the bodies of animals treated in this manner are opened," he informs us, "we find the muscular organs, especially the heart, long irritable, and the intestines long agitated by their peristaltic motion, but the nerves have lost the power of propagating impressions made on them." p. 146. It requires no long train of reasoning to perceive, that these facts would neither lead one to expect benefit from hydrocyanic acid in disorders of the stomach, nor would explain its alleged good effects. The testimony of Drs Elliotson and Granville, to show that it has effected cures of cases of this disorder, appears at first sight satisfactory enough; but, in general, other remedies, as cathartics, were exhibited at the same time, and it is doubtful whether they did not produce the relief which was ascribed to the hydrocyanic acid. Has it any other power than that of diminishing morbid sensibility? Both Brera and Granville recommend and administer this remedy in scirrhus or cancer of the womb; and Manzoni informs us, that Osiander, the late distinguished obstetric professor at Gottingen, has successfully imitated this practice.

We must not omit to mention, however, that this medicine does not appear to retain, among some of the French practitioners, the reputation which it acquired by the researches and experiments of Magendie. Several of them have therefore been prompted to seek for a substitute in one or other of its saline preparations; and it has been thought that they have attained the object of their wishes in the cyanuret of potassium. At the sitting of the Academy of Medicine, of 15th June 1823, MM. Robiquet and Villermé communicated several chemical and physiological observations, to prove that this substance might be advantageously substituted for the pure hydrocyanic acid in the treatment of diseases. * We have already alluded to the inconvenience and uncertainty resulting from the rapid and easy decomposition of this acid. The cyanuret of potassium may be preserved for any length of time without change; and the researches of MM. Robiquet and Villermé tend to show, that its watery solution, or hydrocyanate of potass, retains its properties much longer than the hydrocyanic acid does, and is followed by the same effects on the animal economy as the pure acid. It is well known, these eminent pharmacologists remark, that hydrocyanic acid does not, when pure, possess the faculty of saturating the alkalies; and in the dissolved hydrocyanate, it is free and isolated, as the taste of this solution indicates. But if we trust to its sensible qualities in estimating its physiological powers, is it not to be thought that the free acid shall, in this condition, undergo spontaneous de-

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* Journal de Pharmacie, Vol. IX. p. 279. and p. 370. June 1823.

composition as readily as when no alkaline base is present? This plausible objection does not, according to the observations of MM. Robiquet and Villermé, possess so much strength as might be imagined. They state it as the result of their experiments, that a solution of cyanuret of potassium loses its proper taste, and its power of acting on the nervous system, less rapidly than hydrocyanic acid equally diluted in water. In this respect, they continue, the difference between the two substances is remarkable; the potass retains and fixes the hydrocyanic acid,—but does not saturate it. In support of this assertion, they adduce the following facts.

They made several solutions in like quantities of water, on the one hand of one grain of cyanuret of potassium, and on the other of two drops of the prussic acid, *au quart*, that is, half a drop of the hydrocyanic acid of Gay-Lussac. These solutions were put into similar open vessels, and their taste was compared at the end of six, of eighteen, of twenty-four hours, of two, and of three days; the solutions of cyanuret of potassium always appeared to be less weakened than those of the acid. The difference was particularly conspicuous the first two days, and in the most concentrated solutions, or those which continued exposed to light and sun. They infer, therefore, that hydrocyanate of potass is less readily changed than hydrocyanic acid equally diluted, and that it ought therefore to be preferred for medical use.

A second condition requisite to justify its adoption in medical practice, is the identity of its effects on the functions of the animal body.

When a portion of cyanuret of potassium is applied to the tongue, the spot touched becomes, at the end of one second, the seat of a sensation of fresh coolness, which is quickly changed into a very keen sensation of causticity. If, on the other hand, the tongue is touched with a drop of the solution, the sensation of coolness is experienced at the same instant, while that of causticity is remarked only as the solution is concentrated. In all cases, the solution of one grain of cyanuret of potassium in a given quantity of water, for example half an ounce or one ounce, causes a more keen and more disagreeable sensation, than four drops of prussic acid *au quart*, obtained by the process of Gay-Lussac, and diluted in one ounce or half an ounce of water. This sensation is likewise attended with a very strong taste of bitter almonds, or of their essential oil, and the same burning heat extends along the mouth and throat. Another remarkable sensation is spastic rigidity, a sort of momentary palsy of the tongue, which extends rapidly to the neighbouring parts. Less than a fourth of a grain of cyanuret of potassium, say our authors, or a single drop of its concentrated solution, produced this effect in two persons, while other four did not experience it.

These preliminary trials were succeeded by comparative experiments with hydrocyanic acid, hydrocyanate of potass, and cyanuret of potassium, on birds, guinea-pigs and dogs, performed in the presence of Magendie and others. The general results were poisoning, in the usual mode, by prussic acid, with this difference, that dry cyanuret of potassium, placed on the tongue of animals, with the precaution of keeping the mouth shut, excited there traces of inflammation, or even cauterization. The 10th part of a grain of this salt kills a linnet in thirty seconds after the first symptoms of poisoning appear; and less than one grain kills a large guinea-pig in two or three minutes. One drop of the hydrocyanate, containing only the 100th part of a grain of the salt, kills a linnet at the end of thirty seconds; six drops, containing $\frac{1}{12}$th of a grain, killed a half-grown guinea-pig in less than three hours; and half a *gros*, containing five grains of the salt, killed an old dog, of considerable strength, in fifteen minutes. From these facts, and several others which are not mentioned, MM. Robiquet and Villermé conclude, that the cyanuret of potassium may be advantageously substituted for hydrocyanic acid in the practice of medicine; and that the hydrocyanate, formed extemporaneously by its solution, possesses all the powers which physiologists ascribe to the pure acid. If these inferences be founded on proper and sufficient induction, the cyanuret of potassium must be a valuable substitute; but we have not heard that any observer has yet made researches sufficiently extensive to confirm the conclusions of MM. Robiquet and Villermé. A quantity of it has been recently submitted to trial in the Clinical Wards of the Royal Infirmary; and we may expect some observations towards ascertaining whether it is possessed of medical properties equally powerful with those of the acid, and whether it is an efficient article of the Materia Medica.

We have only to say further, that Dr Granville has given many formulæ for the preparation and administration of hydrocyanic acid. We do not approve, however, of the numerous and varied vehicles in which he directs it to be given. Distilled water or rain water is certainly the most proper, and perhaps the only one that should be used.

Another form of exhibiting hydrocyanic acid has been recommended by Dr Zollikoffer of Philadelphia, who published, in 1822, a treatise on the use of prussiate of iron, or prussian blue, in intermitting and remitting fevers. The preparation which is recommended is called Parisian blue (Pariser-blau), in commerce, and is regarded by Büchner as a ferro-cyanate of oxide of iron. * In doses of from 4 to 6 grains, it is said to be more efficacious in ague than Peruvian bark.

Iodine was discovered in 1813, by Courtois, an eminent manufacturer of saltpetre in Paris; but six years elapsed before it was attempted to be used in the practice of medicine. We learn from the first memoir of Dr Coindet, that in the year 1819, when searching for a formula in the work of Cadet de Gassicourt, he found that Russell had recommended the ashes of the *fucus vesiculosus*, or bladder-wrack, under the name of *Æthiops vegetabilis*, for the cure of bronchocele; and he was led, from analogy between this substance and the burnt sponge, so long famed, to suspect that iodine was the active principle of both. The great and unequalled success which resulted from its use, in the treatment of bronchocele, at once indicated the power of iodine as a therapeutic agent, and encouraged Dr Coindet to pursue his researches in rendering it an efficient article of the *materia medica;* and about the close of the same year, while Dr Coindet had employed iodine in treating goitre for six months at least, his conjecture was confirmed by the discovery which Dr Fyfe of Edinburgh made, that this substance was actually contained in the ashes of the burnt sponge.

It is in medicine, as in other practical applications of human knowledge,—that few innovations entitled to the name of discovery,—few changes distinguished for sagacious conjecture, can be effected, without giving rise to dispute about the merits of the individual with whom the improvement was supposed to originate. It has been generally understood among the profession, that the happy conjecture, which introduced iodine into medical treatment, originated with Dr Coindet of Geneva; yet we find that his claim to this honour is disputed by one of his countrymen, Dr J. C. Straub of Hofwyl, in the Canton of Berne. It is always difficult, and sometimes unsafe, to attempt to decide a question of this sort; and though we have no hesitation in avowing our own personal conviction, as individuals, that the merit of original discovery belongs distinctly to Dr Coindet, yet we shall not so far bias public opinion, as to withhold the principal facts connected with the first employment of iodine as a therapeutic agent.

Dr Straub, whose communication is found in Professor Meisner's "Physical Intelligencer of the General Helvetian Society," &c. for 1820, states, that before the discovery of iodine, attempts had been made to compound a substitute for burnt sponge, but without success; and that this failure, and his observation of the similarity of smell between iodine, burnt sponge, and other marine productions, led him to suspect the existence of iodine, or its salts, in these substances, and that its absence in the artificial compounds was the cause of failure in these experiments. This conjecture, which appears to have been made previous to 1819, led Dr Straub to examine the real burnt sponge; and he informs us, that though his time did not permit him to ascertain exact quantities, yet he obtained from 1½ ounce of burnt sponge, as much iodine as to render his conjecture probable, and to be astonished that the ingredient should have escaped notice. He was therefore at once induced to think of its use in medicine; and, in the same paper from which we obtain these facts, impressed with the poisonous quality ascribed by Orfila to iodine, he recommended first the trial of its salts, especially the hydriodates of soda and of lime, and then that of the substance itself, to physicians whose situation permitted its employment. *

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* Repertorium fur die Pharmacie, Band XVI. heft 2. p. 242.

The communication of Dr Straub is dated December 1819, and was actually published in Professor Meisner's periodical work in February 1820, five months at least before the first memoir of Dr Coindet was communicated to the Helvetian Society of Natural Sciences at Geneva. It is unnecessary to have recourse to any supposition of injustice done to Dr Straub; much less would it be right to deprive Dr Coindet of the merit of originality in substituting the direct and certain action of iodine for the irregular, and sometimes inert, qualities of burnt sponge, in the treatment of goitre. Coincidence of this kind is not uncommon in science; and in the present instance, the ingenuity of Dr Straub does not diminish the merit of Dr Coindet.

But whatever opinion be entertained of the claim to the original suggestion of the use of iodine, there cannot be a doubt that, whatever advantage the practitioner derives from iodine, is mainly due to the exertions of Dr Coindet. The subsequent researches and successful experiments of this physician quickly introduced this remedy to the notice of the profession; and if it was not regularly admitted into the pharmacopœias of all the different countries of Europe, its efficacy has at least been submitted to trial in a very general and extensive manner. The physicians and surgeons of France, of Italy, of Germany, and of England, have, since the publication of Dr Coindet's memoirs in 1820 and

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* Naturwissenschaftlicher Anzeiger der allgemeiner Schweizerischer Gesellschaft, &c. Herausgegeben vom Fr. Meisner, Prof. zu Bern. 4to. Feb. 1820. p. 59.

1821, been zealously occupied, in hospital or in private practice, in ascertaining the powers of iodine, and observing its effects; and though, perhaps, in some respects, it has been misapplied, and in others its virtues have been overrated, it cannot be doubted that it possesses strong claims to the attention of the bold and judicious practitioner.

Among a considerable number of writings which have appeared chiefly on its merits, and on its mode of administration in various forms, the three monographical treatises of Dr Coindet, Professor Brera, and Dr Gairdner, may be supposed to communicate very just views of the real merits of iodine and its preparations. Dr Coindet's short treatise, as translated by Dr Johnson of Bristol, consists of three memoirs, in which the history of its introduction, its several pharmaceutical preparations, its effects in the cure of goitre, and its influence on the economy in general, are clearly and correctly related. It is the most original, and, perhaps, the fairest and most perspicuous statement of the virtues of the remedy. The work of Professor Brera is part of the Report of his practice in the Clinical Institute of Padua for 1820–21. The author not only gives an account of the different cases in which the remedy was tried, but has added a good history of the nature and properties of the remedy, its various preparations, and the formulæ according to which it may be suited to the purposes of the physician. We regard this as the most comprehensive, the most elaborate, and the most scientific account of iodine hitherto published,—and in every respect, except, perhaps, a shade too much of confidence in its powers, worthy of the character of the author. Of the third work we cannot speak so highly. It may indeed be styled, " An Essay, with Practical Observations, " as the author has entitled it; and, perhaps, we must admit that it contains one or two little things which some people would do well to know, about the administration of iodine. Dr Gairdner conceives, that, while the virtues of iodine have been panegyrized by all, its evils have been studiously concealed; and under this impression he has undertaken the task of delineating the horrors of *iodization*, and of execrating with becoming severity the rashness and ignorance of those men, who will administer a powerful and dangerous remedy without watching its effects. To this there can be no rational objection; but when Dr Gairdner monopolizes, as he appears to do, the prescription of iodine, we should fear that many would be disposed to regard the publication of the small treatise before us, as little else than a civil hint to the public,—that, as iodine must be given to cure some diseases, it is unsafe to trust its administration to the common mass of physicians; and that no other but Dr Gairdner is qualified to superintend its action on the living body. That the virtues of iodine are over-rated, that its use has been indiscriminate, we admit, and perhaps shall afterwards show; but we are not certain that Dr Gairdner has any merit in pointing out this, or that he is the only physician who is aware of the abuses to which it is liable, or the means of restraining them.

Iodine may be obtained either from the *mother waters of the soda of vareck*, or from the *soda of vareck* reduced to powder, according to the directions of Wollaston. The mother waters, as they are termed by kelp manufacturers, are obtained by lixiviating the ashes of various kinds of sea-ware *(fuci)* which have been previously burnt, and by concentrating the liquor thus formed.

After Courtois had discovered this substance, Gaultier de Claubry, in 1815, ascertained its existence in many marine plants, and extracted it from the ashes of the sweet wrack *(fucus saccharinus)*, the sea-hangers *(f. digitatus)*, the bladder-wrack *(f. vesiculosus)*, the saw-tooth wrack, *(f. serratus)*, the pod wrack *(f. siliquosus)*, and the sea-laces *(f. filum.)* Davy remarked traces of it in the ashes of the gristly wrack *(f. cartilagineus)*, the pellucid wrack *(f. membranaceus)*, the red proliferous wrack *(f. rubens)*, the thready wrack *(f, filamentosus)*, in the Turkey-feather laver *(ulva pavonia)*, and in the blistered laver (*u. Linza.*) The ashes of the saccharine or sweet wrack, are said to be the best, as a given quantity of them contains a greater proportion of iodine than the same quantity of any other maritime plant. The very extensive shores of Normandy appear to be the most favourable for the growth of *fuci*, from which iodine is to be obtained; Davy examined three specimens of alkaline ashes of marine plants from the shores of Sicily, Spain, and the Roman States, but without detecting any trace of iodine; and it is observed by Brera, that those collected on the coast of the Adriatic, in the neighbourhood of Capo d'Istria, where they are very abundant, furnish iodine only very sparingly. We have been further informed, that the kelp, or impure alkaline ashes obtained by burning the *fuci* of the Scottish shores, contain a very considerable proportion of this substance. It is unfortunate, however, that no exact researches to ascertain the positive quantity have yet been instituted.

Dr Straub of Hofwyl states, that he obtained iodine from the ashes of turf, as it is burnt for domestic purposes; and he thinks it may be found to exist in peat.

Iodine obtained according to the usual chemical process, is a simple substance, analogous to chlorine, solid at the ordinary temperature of the atmosphere, of a grey-black colour, which in a bright light verges to turkey-violet, of metallic brilliancy, slightly tenacious, and with an external aspect between that of native sulphuret of antimony and graphite. It sometimes assumes the appearance of small rhomboidal plates, broad, and splendent, but more frequently like pieces of various dimensions, resembling micaceous iron-ore. A mass of either, when broken, presents internally a laminar and unctuous aspect. Melting at 170° centigrade, and volatilizing at 175° centigrade, under the ordinary pressure of 76 centimetres of mercury, it is converted into violet vapours, which, when collected into a receiver, are again condensed in octahedral crystalline plates, the axes of which have been determined to be to each other in the ratio of 2, 3, 4. Its odour is disagreeable, like that of chlorine; the sensation which it leaves on the tongue is sharp, hot, and durable; it leaves on the skin a yellow stain, which readily disappears; but tinges white paper with a reddish white spot, which finally corrodes the part.

Iodine unites with oxygen and hydrogen, and forms substances with acid properties to which the names of *iodic* and *hydriodic* have been respectively given. The iodic acid combines with the metallic oxides, forming iodates of potassa, soda, baryta, lime, zinc, &c. Professor Brera describes the mode of preparing these iodates, and their chemical and physical qualities, as he thinks the physician will hardly have occasion for more varied products. Any of these dissolved in water is found to be capable of uniting with an additional proportion of iodine, and thus forming *ioduretted iodates.*

The hydriodic acid combines in like manner with the metallic oxides, and forms hydriodates, which are more familiarly known than the iodates. The pharmacology of these salts given by Brera is very comprehensive, and, like that of the iodates, includes varieties which have not hitherto been generally known in medical practice, and which perhaps never will come into general use. After stating that the knowledge of the hydriodates is most important to clinical practitioners, because some have been used with the greatest success, and others promise to furnish precious therapeutic results, he limits the principal to the number of six,—the hydriodates of potass, of soda, of baryta, of lime, of magnesia, and of zinc. In point of fact, we believe none but the two first have been prescribed either in France or in England; and Professor Brera's pharmaceutic list contains no preparation of the others. It is unnecessary to dwell here on the chemical details of these several preparations. Better information of this description than any which we could give or transcribe, is found in the recent chemical works of Vauquelin, Thenard and Thomson, and in the Clinical Essay of Brera, now before us. All that we need say is, that it is a characteristic of all these hydriodates, that the watery solution of each is capable of dissolving a considerable quantity of iodine, and thus forming *ioduretted* hydriodates of their respectives bases. The iodine, in these ioduretted solutions, appears to adhere weakly, for it is detached by boiling, or by mere exposure to air, when the iodurated hydriodate is undergoing exsiccation. It is further remarkable, that the iodine does not alter the neutral state of the hydriodates. These solutions acquire in the addition a red-brown colour, similar to that of the simple solution of iodine; and this is further regarded by Brera as a proof of the weakness of the combination. Whatever be the case, it is found by actual experience, that the ioduretted solutions of potass and soda are at once the most convenient and the most efficacious forms in which iodine can be administered.

Of the other combinations of iodine, Professor Brera informs us, that he has hitherto used only those of mercury; and that the ioduret of *amidon* (starch) is a preparation which may be useful, as it enables the physician to introduce iodine into the system by slight dry frictions, practised in the manner of the mercurial ones of Clark, or the auriferous ones of the method of Westring.

The proto-ioduret of mercury is a green yellow substance, insoluble in water and alcohol, but changeable in light. It consists of 250 parts of mercury and 156.25 of iodine; so that $8\frac{1}{10}$ grains of proto-ioduret contain $3\frac{1}{10}$ grains of iodine; and one grain of iodine is contained in $2\frac{61}{100}$ of proto-ioduret. Its specific gravity is 7.150.

The deuto-ioduret of mercury is of a bright red colour, insoluble in water, but soluble in alcohol, ether, acids, and the alkaline hydriodates. It is formed by the union of 250 parts of mercury, and 312.50 parts of iodine, or 5 parts of mercury and $6\frac{2}{10}$ of iodine. One grain of iodine is contained in $1\frac{84}{100}$ grains of this deuto-ioduret. Its specific gravity is 4.685.

If *amidon* (starch) be treated with excess of iodine, and the mixture dissolved in potass, the addition of a vegetable acid is followed by a precipitate of a fine blue colour, which is ioduret of amidon. Amidon is the most powerful reagent for detecting the presence of iodine; and Stromeyer of Gottingen states, that this substance, introduced into a fluid containing only $\frac{450}{1000}$ or $\frac{9}{20}$ of iodine, instantly attested its presence by the blue tint which it caused.

It appears, from the memoirs of Dr Coindet, that he exhibited iodine in three different forms; 1*st*, alcoholic or ethereal tincture of iodine; 2*d*, ioduretted hydriodate of potass in solution; and, 3*d*, in the form of *pommade* or ointment. The formulæ for these preparations are given in the Formulaire of Magendie. Professor Brera has given, in the present work, *formulæ* for pharmaceutical preparations; 1*st*, of iodine; 2*d*, of hydriodic acid; 3*d*, of iodine in the form of hydriodate; 4*th*, of iodine in the form of the mercurial iodurets.

Under the first head we find the four following preparations; 1*st*, iodic alcohol, or tincture of iodine, which is prepared, by dissolving 48 grains of iodine in one ounce of alcohol at 35° of Beaumé's araeometer, or .852 density. This preparation is evidently copied from that of Dr Coindet, and it is therefore said, that 20 drops contain one grain of iodine. But, as the Poids de Marc ounce, which is used in Geneva, contains 576 grains, and the Troy ounce, which is used in almost all other countries, contains only 480, 48 Genevese grains are equivalent to 40 Troy, or, in other words, $\frac{1}{12}$ of iodine by weight, and not $\frac{1}{10}$, is to be dissolved in the ounce of spirit to make the proper tincture. 2*d*, Ioduretted sulphuric ether, formed by dissolving six grains of iodine in one drachm of ether at 66°, giving one grain of iodine in 30 drops of the solution. 3*d*, Iodine pills, two of which are made by rubbing one grain of the substance with a sufficient quantity of *rob* of elder and liquorice powder. 4*th*, Iodine ointment (*Pomata di iodio*), formed by rubbing half a drachm of iodine with 1½ ounce of the purest lard.

The only preparation of the acid is hydriodic ether, obtained by distilling a mixture of two parts of alcohol, and one part, by volume, of hydriodic acid, at 1.700 density, until an alcoholic fluid, perfectly neutral and limpid, is perceived.

The preparations of hydriodate of potass given by Brera are as follow; 1*st*, solution of hydriodate of potass, formed by dissolving 36 grains of the salt in one ounce of distilled water. 2*d*, Solution of ioduretted hydriodate of potass, made by adding 10 grains of iodine, and 2 drachms of water, to the foregoing solution. The two drachms of distilled water are peculiar to Brera, for neither Coindet nor Magendie direct this, nor do we regard it as essentially necessary. 3*d*, Ointment of hydriodate of potass, by combining half a drachm of the salt with 1½ ounce of lard;—the same as Coindet's ointment, and the *pommade* of Magendie's formulaire. 4*th*, The ioduretted gastro-iodated ointment of potass (*Pomata gastro-iodata di potassa iodurata*). This polyonymous ointment is prepared by mixing one drachm of hydriodate of potass, and one scruple of iodine, with a sufficient quantity of depurated calf's gastric fluid, for digesting in 24 hours, and then rubbing the whole with a proper proportion of lard. This ointment, which has undoubtedly the merit of being original and sufficiently singular, is intensely yellow-red, and is said to be the most safe, mild and efficacious mode for the external application of iodine hitherto used.

Under the head of iodurets, we find, 1*st*, alcoholic solution of deuto-ioduret of mercury, obtained by dissolving 22 grains of the salt in 1½ ounce of alcohol at 35°. This medicine acts energetically on the stomach; and it should not be given at first in greater quantity than 10 drops at a time, which contain $\frac{1}{15}$ of a grain of the deuto-ioduret. 2*d*, Sulphuric ether, with deuto-ioduret of mercury, made in the same manner as the last. And, 3*d*, pills of proto-ioduret of mercury, eight of which are formed by combining one grain of the salt with a proper quanty of elder-rob and liquorice powder.

Of all these preparations, the safest, according to the observations of Coindet, is the ioduretted hydriodate of potass, and next to that the alcoholic or ethereal tincture. Brera employs not only these preparations, but iodine in the form of pill or bolus, the alcoholic tincture of deuto-ioduret of mercury, and the gastro-iodate liniment, to which he is very partial.

It was in the treatment of goitre that Coindet observed the powerful effects of this remedy as an agent; and he was led, perhaps too hastily, to infer, that it would have similar beneficial results in strumous swellings. It is remarkable, that this judicious physician was quite aware of the fallacious views which ally goitre and king's evil; for he observes, that practitioners have been deceived by the name of *gland*, which has long been applied to that body, the enlargement of which constitutes the swollen throat (*tumidum guttur, gozzo*) of the Alps. Yet he afterwards informs us, that its powerful action on the absorbent system induced him to employ it in scrofula uncombined with fever, and where the enlarged glands of the neck were indolent. His own success, and that of several brother practitioners of Geneva, confirmed his views, and perhaps led to an extreme confidence in its powers. We understand, from the personal observation of a friend on whose accuracy we can place the utmost reliance, that while he was in Geneva last summer, it was by no means found to answer the sanguine expectations which were once entertained of its effects on the management of affections ascribed to the strumous disposition. We have ourselves witnessed several very good cures of scrofulous glands under its use; but in most of them suppuration took place; and it may be doubted, whether the iodine exercised its effects on the absorbent system, or enabled the organic vessels of the glands to assume a suppurative action of a more healthy character.

The Clinical Essays of the Paduan Professor contain thirteen cases of various kinds of disease, in all of which, except one, a cure was effected under the use of some preparation of iodine. One case of incipient mesenteric tabes, two cases of vicarious hemoptysis, both in females with suppressed menstruation, one case, said to be vicarious dysentery, three cases of chlorosis, with a variety of symptoms, one case of amenorrhœa, one of difficult menstruation, two of enlargement of the submaxillary glands, ascribed to a scrofuloso-syphilitic taint, and one case of syphilitic bronchocele, were treated by this remedy with favourable issue. A desperate case of laryngeal consumption terminated fatally. We have not, at present, leisure to inquire into the circumstances of all these cases; nor do we feel much inclined to scrutinize with severity the cases of the learned Professor. But we should be rather sceptical with regard to the favourable effect of iodine on diseases like inflammation of the mesenteric glands, or pulmonary hemorrhage. A letter from Dr Francis Marcolini of Udina to Professor Brera, communicates the particulars of three cases of scrofulous glandular disorder, and one of goitre, treated successfully by this remedy. This is an example which may be recommended to those practitioners who reside in districts where glandular enlargements are frequent; and if their observations are accurate, the merits of the remedy might be determined without much chance of mistake.

An analogy, which has never appeared to us well established, and the truth of which has been admitted by the profession rather on the authority of doctrine and opinion, than on that of sufficiently extended observation, appears at present inclined to adopt the use of iodine in the treatment of consumption. The analogy to which we allude, is that supposed to exist between glandular scrofula or the strumous diathesis in general, and tubercular destruction of the pulmonic tissue. It would be an object of some consequence to the precision of pathological doctrine, to see this analogy confirmed or disproved; and it would be a much more happy circumstance, if it could be shown that iodine prevents the formation of tubercles, or removes them when formed. But not only is this improbable, we had almost said inconceivable; but there are circumstances in the action of iodine, which strongly and imperiously forbid its use in diseases in which the pulmonary functions are in the most trivial degree deranged. One of the effects of *iodization*, or that state induced by charging the system to excess with iodine, is painful constriction of the chest, with some degree of cough and disordered respiration. This led Dr Coindet to mention incipient diseases of the chest as one of the states in which the exhibition of iodine is contraindicated. (p. 23.) Yet we find that Dr Gairdner, amidst his affected discrimination of the proper use of this remedy, has employed it " in cases where he had good evidence of the presence of tubercles in the lungs, and that he does not doubt that it will be found serviceable in the incipient stages of the disease. " (p. 52.) We should like to know what this evidence was; and what circumstances lead Dr Gairdner to recommend this remedy in incipient consumption, any more than in the advanced state. Medicine is unquestionably a science of observation, and an art of experiment; and we should be loth indeed to substitute *a priori* assertions and principles for the results and evidence of actual fact. But it is certainly not a proof of the closest adherence to consistency, for Dr Gairdner to commence his work with strictures on the ignorant and indiscriminate practice of using iodine in all and sundry forms of intractable disease,—and to conclude it by telling the world that he has removed pulmonary tubercles by its exhibition, and that he thinks it serviceable in the early period of consumption, when it is well known the symptoms of pulmonary irritation are considerable. Neither the evidence of Dr Baron, nor that of the late Mr Haden, are sufficient to convince us of the propriety or the safety of using iodine under such circumstances. We cannot perceive how the positive existence of tubercles can be ascertained, unless the lungs of the individual, destroyed by some other disease, were to present them or their traces after death.

On the whole, we regard it as established by the evidence of Dr Coindet, that iodine may be used with benefit and success in goitre, especially that form of it which prevails in Switzerland, Piedmont, and the lower and maritime Alps; and that it may be advantageous in the treatment of some other indolent tumors, which are not very hard or tuberculated, whether they be a new formation, or consist in enlargement of glandular bodies already existing; but it still remains to be proved, that it can be administered either with safety or benefit in any process of internal disorganization; and certainly it ought not to be prescribed either in consumption or in any other tubercular disease.

We should have spoken more fully of that train of symptoms which result from over-charging the system with iodine; the *idiodic* symptoms of Coindet, or what has been named by Brera and others, *iodization.* But we must put a conclusion to this article, which is already too long,—and be satisfied by referring our readers to the Treatise of Dr Gairdner, which contains an excellent and interesting case of this dreadful condition ;—the best part of the Essay.

PART III.

MEDICAL INTELLIGENCE.

DOMESTIC.

Diseases of Birmingham, from January 3. *to April* 3. 1824.

ACUTE DISEASES.

	No.		No.
Cephalalgia	13	Gonorrhœa	3
Dentitio	15	Synochus	23
Ophthalmia	6	Typhus	9
Cynanche Parotidæa	4	Rheumatismus A.	8
Aphthæ	2	Arthritis Rheumatica	5
Cynanche Tonsillaris	1	Febris Infantum	9
Pneumonia	4	Abscessus Mammæ	1
Pleuritis	1	Mastodynia	1
Dyspnœa	1	Contusio	2
Obstipatio	52	Erysipelas	4
Colica	6	Urticaria	2
Diarrhœa	16	Rubeola	7
Peritonitis	1	Herpes Zoster	1
Enteritis	2	Variola	1
Hepatitis A.	1	Furunculus	1
Mæmorrhagia	14		

CHRONIC DISEASES.

	No.		No.
Cephalæa	19	Hepatitis C.	14
Epilepsia	2	Icterus	10
Mania	1	Amenorrhœa	9
Hysteria	2	Carcinoma Uteri	1
Amaurosis	1	Dysuria	1
Hemicrania	2	Hernia Humoralis	1
Neuralgia	1	Hydrops Generalis	1
Bronchitis C.	18	Rheumatismus C.	14
Asthma	60	Lumbago	2
Hæmoptysis	4	Coxalgia	2
Hydrothorax	6	Lichen	4
Tussis	13	Prurigo	1
Phthisis Pulmonalis	68	Strophulus	3
Aphonia	1	Porrigo Larvalis	5
Palpitatio	3	Porrigo Favosa	6
Salivatio Gravidarum	1	Impetigo	2
Tonsillæ Tumidæ	1	Scabies	2
Asthenia	13	Psoriasis	1

Dyspepsia	113	Lepra	3
Pyrosis	1	Nævus	1
Gastrodynia	12	Ulcus	1
Hæmorrhoides	1	Varices	1
Marasmus	28	Tumor Subter Cutem	2
Cachexia	6	Struma	1
Lumbrici	3	Dolor Inguinis	1
Tænia	11	Abscessus	1
Ascarides	1	Exostosis	1
Paralysis Partialis	1		
Hemiplegia	1	Total	702
Fistula Ani	1		

The last three months have been generally mild, although wet; and, till the beginning of March, we had very little frost or snow.

The pulmonary affections have exhibited a marked difference from the corresponding quarter last year, both in their numbers and severity. From January to April 1823 they bore a proportion of nearly one in three, and among these there were very many fatal cases. In the same months, in 1824, their proportion is one to four, and for the most part very slight. Towards the latter end of the quarter there have been some instances of pneumonia after measles, and not unfrequently fatal from neglect.

Though the list contains only seven cases of measles, this cannot be taken as any criterion of their frequency; both this disease and small-pox are usually left to their own course among the poor; and it is too often by their sequelæ that we become acquainted with their prevalence. These have been various and serious. The cases of pneumonia frequently ending in consumption, or being combined with hydrocephalus or marasmus. Very many of the cases of marasmus were consequent to measles.

Among the dyspeptic complaints, the principal circumstance worthy of notice, has been a severe and nearly constant pain along the whole course of the colon, but most intense in the transverse arch of that intestine. In some instances, the patients were subject to fainting fits, especially in the exacerbations of the pain. Sometimes these fits were the principal complaint; and it was not till after examination that the patients themselves were aware of their origin. In one individual, the fits were very frequent, and were found uniformly to occur oftener, as the pain was more intense. Upon the application of leeches, they have always become less urgent, and as commonly returned a few days after the bleeding. The bowels have at the same time been very costive. She appears now to be recovering, under the use of very strong purgatives, with six grains of calomel daily, which, however, do not operate more than twice a day. In another case, of less duration, the pain was referred to the situation of the caput coli, and the faintings often endured for an hour or more. Here the disease quickly yielded to the repeated application of leeches.

One of the cases of erysipelas occurred in a child of two years of age, and was an excellent illustration of the value of Dr Hamilton's remarks on purgatives. The patient was purged regularly every day for a week, with jalap and calomel, without procuring watery evacuations, but with the dejection of offensive slimy matter, frequently mixed with scybala. She recovered under this treatment, without further medicine.

Catarrhal affections became frequent towards the latter end of the quarter, and were generally accompanied by very severe cough. The influence of the NE. wind was remarkable in retarding the recovery of such patients; while a change to the NW. or SW. almost instantly effected an alteration for the better.

The case of psoriasis appeared to have originated in great mental distress; it was very severe upon the arms, and was more or less general over the whole body. She has perfectly recovered with aperients and tonics.

Several additional instances of success from acupuncturation have occurred this quarter.

Trial for Poisoning by Sulphuric Acid.—The following case is one of the best examples we have met with of the importance and efficacy of medical evidence in cases of murder by poisoning, when carefully collected by qualified persons. It does great honour to the medical gentlemen who were concerned in it; and will afford a pleasing contrast to the crude and unsatisfactory investigations which it is too often our lot to witness in judicial proceedings. The case is taken from a newspaper report (London Courier, 22d March), but it bears in every important particular the stamp of authenticity. Richard Overfield was indicted at the Shrewsbury assizes, on the 19th of last March, for administering sulphuric acid to his own child, an infant three months old. About 8 o'clock in the morning of Sunday, 21st September, the child was seen in perfect health by a woman in the neighbourhood; but between 11 and 12 her attention was caught by the screams of its mother; and on entering the house, she found it apparently in great agony, and the mother holding it to her breast. Mrs Overfield soon afterwards carried the child to a surgeon's She was for some time in such a state of agitation, that she could not tell her errand; but at length she pointed to the child's lips, and desired him to put his tongue to them. He depones, "I did so, and found a very pungent acid taste, which no female milk could give. The lips were white and shrivelled, and had small blisters on their inner surface; and there were similar appearances on the mouth and tongue. The child was in violent agony, and tossed itself about, so that it was difficult to hold it steady." Concluding that it must have swallowed some acid, and probably oil of vitriol, he gave it magnesia suspended in water. He then attended the mother home, and there examined into the circumstances of the case more minutely. "I examined, continues he, the clothes of the child, and those on which it lay; and on the child's frock (made of dyed cotton) I saw two or three red spots some inches from its mouth. In order to ascertain if those were recently made, I put my tongue to each, and each communicated an acid taste, such as I had felt in its mouth. About one the infant became unable to swallow; soon after it breathed with difficulty and noise; the extremities grew cold; and at three it expired." Another surgeon, the partner of the former, who saw the child at intervals, also depones to all the leading circumstances just mentioned. They examined the body together. "The lips were internally blistered, and externally of a dark colour; the gullet was contracted, and the internal lining corroded. The membrane lining the mouth and tongue was of a dull white colour. On laying open the cavity of the abdomen, a bloody coloured fluid escaped; nearly a pint which remained was preserved for analysis; and it proved to contain a considerable quantity of sulphuric acid, quite sufficient to occasion death. The great curvature of the stomach was corroded, and reduced to a dark substance resembling wet brown paper." (The expressions are not altogether clear; but we presume the stomach must have been perforated. The cause of the extensive injury of the stomach appears to have been, that the child never vomited.) A part of the contents of the stomach was likewise analyzed by a physician of the town of Shrewsbury, who agreed with the other witnesses as to the existence in it of a quantity of sulphuric acid, more than sufficient to occasion death.

The crime was traced most satisfactorily to the prisoner by various points of moral evidence; of which we shall mention one only, as it was in part supplied by the medical examination. He worked in a carpet manufactory, where he had access to the factory stores of sulphuric acid; and from the evidence of the superintendent, it is clear he understood its properties, though he repeatedly denied also either that he knew what it was. On the day of the child's death, about one o'clock, his next-door neighbuor observed him walk down his garden, stoop close to a paling fence, and then return to the house. Next day another woman, while walking with the child's mother in the garden, found a phial lying on the other side of the paling just where the prisoner had been seen to stoop. This contained a tea-spoonful of a black liquid, which smelt very strong, and which was found by the medical witnesses to contain a large proportion of sulphuric acid. He did not attempt any defence. But on being asked before the trial what he had done to the child, he repeatedly said that "he had only knocked away the black cat, which had got up to the child's mouth, and brought foam up." The black cat was procured not long after the child was first observed to be ill, and the surgeon examined its mouth and paws, but could not detect any traces of sulphuric acid on them.

Trial for Murder by Blows on the Head.—We have received from Mr Murray of Alford in Aberdeenshire, an interesting communication on the medical evidence brought forward on the trial of Charles M'Ewan, who was executed here last April for the murder of a woman. We should have been anxious to have related the case at full length; but could not have done so without entering into details, which our limits will not permit, and which, besides, would have wanted much of their intrinsic interest from the absence of some points of information, which cannot now be supplied. We shall therefore mention only the leading particulars of the evidence, so far as they are requisite to illustrate the opinions given by Mr Murray on the trial, and to remove any unfavourable impressions that may have resulted from the unjustifiable distrust which these opinions appear to have been received by the Court. The dead body of the woman was found in a lonely part of a road in Aberdeenshire, in October last, on the morning after she had been seen alive in the prisoner's company. A considerable quantity of blood was observed around it; and on the head and upper part of the face there were seven contused wounds, four of them curved, and from two to three inches long, three straight and slighter; and none of them had injured the subjacent bone. About half an ounce of fluid blood had been effused between the skull and dura mater (at what particular point our correspondent has forgot to mention.) The stomach contained a quantity of fluid contents, apparently spirituous. Such were all the appearances of note found on the examination of the body, on the 15th October, by Mr Murray and Dr Garioch, the medical inspectors. Fourteen days afterwards, the body was disinterred, and the wounds again examined by the latter witness alone, for the purpose of comparing them with a portable anvil found in the prisoner's possession, and supposed to have been the instrument of murder. This anvil was of a peculiar shape at one of its sides, and Dr Garioch was of opinion, that two of its edges, of a concave form, corresponded with some of the wounds. Mr Murray, on the other hand, who had been absent at a distance when the second examination was made, deponed that, "although a certain part of the instrument might make an impression similar to the wounds, yet he did not think it *probable* the anvil could have been the weapon used, for the shape of its edge did not correspond with the respective depths of the several parts of the wounds." And he stated further, that "he was then (at the trial) less strongly inclined to believe that the anvil was the instrument of murder, than on a former occasion, when it was first shown to him; because he had previously a less exact impression on his mind of the nature of the wounds." The following opinion as to the cause of death was given by both witnesses, in their report at the precognition, and adhered to, with some trifling variation on the part of Mr Murray, at the trial. "Though we are therefore unable to specify with precision the more *immediate* cause of death, we believe this to have arisen from the wounds; which opinion we found partly on the nature of these wounds, and partly on the absence of any other circumstance sufficient to account for death." Perhaps, this opinion, especially after the preamble with which it sets out, is a little too strongly expressed. Wounds of the mere integuments of the face and head, accompanied with so little internal hemorrhage, could have occasioned speedy death in one of three ways only; *first*, by causing fatal external hemorrhage, which was not proved either by the quantity of blood actually shed, or by the signs of exsanguification in the body itself; * *secondly*, in consequence of being accompanied with violent concussion of the brain, which, considering the character of the wounds, was not probably a concomitant effect of the blows, and which, besides, though it had been produced, could not have been recognised in the dead body by any signs with which we are acquainted; or *thirdly*, in consequence of the cooperation of exposure to cold during the night, or her probable state of intoxication, or both together. This last appears to have been the most probable state of the matter. But, at all events, *no other cause of death having been detected*, a surgeon cannot but believe that the wounds must have been intimately connected with it in some way or other, since they must have been inflicted a very short time before it, and since the woman appears, from various circumstances, to have never moved from the spot where they were inflicted. The only part of the evidence, however, with which the Court expressed its dissatisfaction, was the opinion of Mr Murray as to the improbability of the wounds having been inflicted by the anvil, together with his statement, that he had more doubts upon the matter at the trial, than when the anvil was first shown to him. The Judge represented himself as unable to comprehend how such a change could take place in any man's sentiments; and commented on Dr Murray's evidence in terms peculiarly galling, and certainly most unnecessary. No medical man—*no* ordinary person of common intelligence should be at a loss to conceive, how his professional sentiments may undergo a most essential change on mature reflection. On this point, therefore, we need say nothing. As to Mr Murray's actual opinion at the trial, we must remark, that we cannot well imagine how such an instrument as a portable anvil (the lightest of which weighs 3 or 4 pounds), in the hands of a man of so great muscular power as the prisoner, could have inflicted wounds accompanied with so little contusion, and altogether of so trifling a description as those found on the body of the deceased And the reasons assigned in Court by the witness, if not sufficient to establish his opinion of *improbability*, were at all events substantial enough to entitle his statements to be received with the civility and respect due to a person of liberal education, who could have no possible interest in the case, and who had been commended by the Bench for his luminous evidence on several previous occasions. It was, besides, in our humble opinion, a point of little consequence, whether the anvil fitted the wounds or not, because they were evidently of such a nature as to have been also producible by a variety of other instruments. We may add, that the prisoner before his death, confessed his guilt, alleging, however, that he had committed the deed while in a state of intoxication, and ignorant of what he was doing. No notice was taken in the public prints of the manner in which he supposed he had done it; but Mr Murray learned, by applying to the clergyman who attended him, that "he was inclined to believe the wounds were made with a stick he carried, but was quite sure the anvil was not the weapon, because the box containing it had not been opened."

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* The neglecting to ascertain the fact regarding this point, as Mr Murray in his communication allows, was a material defect in the medico-legal inquiry. It appears to have arisen from the unfavourable circumstances under which the body was judicially examined.

Attempt to Poison, by administering Pounded Glass.—An idea prevailed long among medical men, and is still common among the vulgar, that pounded glass and small fragments of other hard and sharp-edged substances are poisonous. But recent experiments have shown the reverse. It is undoubtedly true, that if such substances as broken glass be swallowed in fragments of considerable size, they may produce serious injury by tearing and cutting the membranes of the alimentary canal, or by lodging in some part of it, and there constituting a source of perpetual irritation. Hence persons have been injured by attempting the wager of eating a wine-glass. Yet even that extraordinary amusement has been sometimes indulged in with impunity; of which an instance may be seen by referring to the article Spontaneous Combustion in the Foreign Intelligence of the present Number. As for glass reduced to fine powder, or even to small fragments only, repeated experiment on animals has proved, that it is absolutely inert. In an Inaugural Dissertation, published at Paris in 1820 by Le Sauvage, it is stated that two drachms and a half were given to a cat without injury; nay that a dog took six or seven ounces in eight days, without suffering the slightest inconvenience, although care was taken to administer it always while he was fasting, and although the fragments were frequently a line in length. Emboldened by these results, the experimenter himself swallowed a considerable number of similar particles, without sustaining any inconvenience. Dr Paris, in his work on Medical Jurisprudence, has referred to the third volume of the Memoirs of the Academy of Pavia for an account of some experiments to the same effect by Mandruzzato, an Italian physician. The qualities of this substance have come under the review of Courts of Justice. Dr Beck has given an abstract of a report by Professors Baudelocque and Chaussier, delivered at Paris in 1803, on the occasion of a trial for murder, in which the accused was supposed to have poisoned his wife with pounded glass. This substance was actually found in the stomach of the deceased; and the stomach, as well as the intestines, exhibited signs of great irritation. After a careful investigation of all that had been written by authors on the matter, they give their opinion, that pounded glass is not a poison; and suggest whether it might not have come from some glass vessel broken by her teeth during the convulsions which preceded death.

We have given these short statements as an introduction and commentary on a short notice we have received from Dr W. Turner, at Spanish-Town, Jamaica, of an attempt made by a negro woman to poison a whole family with pounded glass. The persons on whom the attempt was made were seven in number, the mistress of the family, four children, and two servants. A large quantity of the pounded glass was put into a dish of curried fowl, of which every one partook liberally. The fact being discovered towards the conclusion of the meal, excited much alarm; and the master of the family, who is a chemist and druggist, immediately gave all of them purgatives. In consequence, they all passed large quantities of coarsely powdered bottle glass. When Dr Turner saw them four days after the attempt, none of them had suffered any inconvenience. This result amply confirms the opinions of the French Professors and the experiments of Le Sauvage.

Monument to the Memory of Dr Baillie.—No individual of the medical profession has ever received such posthumous honours from his brethren as the late Dr Baillie. The Colleges of Physicians and Surgeons of London have each of them voted a bust, to be executed by Chantry, and placed in their halls; and the Medico-Chirurgical Society propose to have a portrait of him for their library. The members of the medical profession throughout London have likewise resolved to set on foot a subscription, with a view of erecting a monument to his memory in St Paul's or Westminster Abbey; and at the last general meeting of Subscribers on the 28th April last, the amount of subscriptions was announced to be 700*l*. We need scarcely add, that the list already includes many of the most distinguished names in the metropolis. A committee of 21 members has been chosen for promoting the subscription and carrying its intention into effect. It is composed of Drs Maton, Warren, Babington, Hooper, Gooch, Paris, Latham, Somerville, Yeats, Sir J. MacGregor, Sir A. Cooper, Messrs Brodie, Pennington, Tupper, C. Bell, Clarke, Hollings, Alexander, Cline, Travers, Abernethy. All members of the profession throughout the United Kingdom are invited to second the Committee's views; and no doubt is entertained, that the required amount will be shortly completed.

Annual Report of the Benefit Society for Widows of Army Medical Officers.—The ninth annual meeting of this excellent institution was held in London on the 14th of last May, when it was reported that the Society's Capital amounted to L.31,447, vested in the Three per Cents Reduced; that during last year, their receipts, from dividends, subscriptions and marriage fines, amounted to L.3125, and that the number of members is 592, of whom 182 are unmarried. The number of widows on the fund during last year was seven. This Society was instituted in 1816, for enabling the medical officers of the Regular Army, of the Regular Militia, and in the East India Company's service, to effect insurances on their lives for the benefit of their widows. There are two classes of subscribers; the widows of the higher class receive an annuity of L.40, those of the lower an annuity of L.20. A subscriber of the first class pays five guineas annually if married. If unmarried he pays two guineas, and on his marriage a fine of twenty guineas; which, however, is diminished to one-half, if he has subscribed ten years, and is taken off altogether if he has subscribed twenty years. Besides, every such subscriber, whose age exceeds that of his wife by five years, pays for every year beyond that limit another fine, namely L.2 if he is under 30 years of age, L.2 14s. if he is under 40, L.3 15s. if he is under 50, L.4 10s. if he is under 60, and L.6 if he is under 70. Subscribers of the second class pay one-half of these annual subscriptions and fines; and they may be admitted, with the consent of the committee of management, subscribers of the first class. Before a widow can be entitled to an annuity, her husband must have been a member five years, and made six annual payments. Subscriptions are due on the 24th January; if not paid within three months, the subscriber is expelled. If any member shall subscribe as a Bachelor more than six months after marriage, his previous payments are forfeited, and his widow derives no benefit from the fund.

Army Medical Officers' Benevolent Fund.—This is a charitable institution set agoing about three years ago, for affording relief to the widows and orphans of the medical officers of the army. The fund is raised by voluntary subscription. It is proposed to allow it to accumulate till it yield a permanent income of L.100. At the last annual meeting held in London on the 14th of last May (Sir J. MacGregor in the chair), it was stated, that the interest of the property already vested amounts to L.73 10s. "The Committee of Management urge upon the General Meeting the necessity of continuing their exertions in behalf of the fund. It is not perhaps generally known, that the objects of the Society's bounty are numerous, and that very many widows and orphans are anxiously waiting for the period when its benevolent operations may commence. The hope held out in the last Annual Report, that a distribution of the interest of the stock might possibly take place in the course of the present year, slight as it was, induced several most deserving persons to forward to the Committee their claims; and it was with great pain that the Committee were compelled to withhold from them any prospect of immediate assistance. The fund, however, has now reached that point, when a slight addition to the united zeal of its friends will enable it to commence its career of active charity; and the Committee are well assured, that an appeal thus addressed to the liberality of their brother officers will not be made in vain."

FOREIGN.

On the use of the St Ignatio Bean as a Remedy for Epilepsy. (De Faba Sancti Ignatii Commentatio, Scripta a Gul. Andr. Haasio, in Univ. Lips. Therap. et Mat. Med. Professore, &c. 1822.)—It is well known that this substance, the fruit of a species of strychnos, is one of those in which Pelletier and Caventou lately discovered the vegetable alkali, the strychnia. It appears to have been first introduced to the general notice of the profession in 1699, in consequence of a paper published in the Philosophical Transactions for that year, by a Jesuit, Camelli, who had found it to be a favourite remedy for many diseases among the inhabitants of the Phillippine Islands. For a long period it had been almost forgotten, till the interesting researches of the French chemists brought it again before the public eye. It appears, however, that a German physician of Numberg, of the name of Weitz, had long employed it as a secret remedy against epilepsy, and with such success as obtained him considerable reputation for the cure of that disease, and even induced practitioners at a distance to send their patients to Numberg to be put under his care. The secret was handed down by him to his son, by whom it was entrusted to W. A. Haase, professor of materia medica in Leipsig University, on condition that it should not be made public till after young Weitz, and his father, were dead. Both of them having died not long ago, and Professor Haase having confirmed their opinion of the remedy by some trials made lately, he has published a short account of its history as an article of the materia medica, and his experience of its virtues in epilepsy and some other diseases. The cases of epilepsy he has published, are five in number. The first was that of a girl 20 years old, whom he sent to be cured by Weitz himself. She had been first seized with epilepsy in her 18th year; the paroxysms recurred at the beginning once every two months, but afterwards she had them once or twice a-week. After being put under the care of the physician of Numberg, she had only one paroxysm, and never had another from that period, down to the date of Haase's publication, 18 years afterwards. The second case was that of a young man of sedentary and literary habits, who had frequently had epilepsy in his childhood, but remained tolerably free from it till betwixt his 20th and 30th year. At the latter period, it attacked him more violently. Haase then gave him the St Ignatio Bean; under the use of which the paroxysms became gradually less frequent, till at last they ceased to return. The third patient was a man 50 years old, liable to frequent paroxysms since his 18th year, the consequence, apparently, of masturbation. His mental faculties were much impaired by the disease; his senses had become dull, and his bodily frame languid. In him the remedy greatly diminished the frequency of the disease, but did not dispel it altogether. The fourth case likewise arose from the same cause, conjoined with habitual drunkenness. The epileptic paroxysms were removed in two months; but he died a year afterwards of *tabes dorsalis*, which had existed at the time he was first put under treatment. The last case was one of epilepsy from worms, which appears to have been cured by the expulsion of the worms effected by the St Ignatio bean. These cases, although certainly few in number, are perhaps sufficient to encourage the farther trial of the remedy. It must be used with some caution, however, as its poisonous properties are of the most formidable kind. The dose given in the foregoing cases was two or three grains twice or thrice a-day. Professor Haase also warns his readers, that it will not be of use in those cases of epilepsy which originate in cerebral disease, or intestinal irritations, but in those only which arise from general impressions on the nervous system, the effect, for example, of violent passions, hysteria, or onanism.

Treatment of Peripneumony by Tartar-Emetic. (*Arch. Gén. de Méd. Fevr.* 1824. iv. 300.)—The following is an abstract of the most authentic account yet published of the present Italian mode of treating peripneumony by large doses of tartar-emetic. It is taken from the translation of a memoir by Rasori, professor of clinical medicine at Milan, and one of the most strenuous advocates of the system. Rasori considers, that the capability of enduring large doses of tartar-emetic, without the supervention of vomiting, or the other customary signs of its action on the alimentary canal, is peculiar to the diseased state of the system; and, consequently, as soon as the system begins to return to its healthy condition, the peculiarity ceases, and the drug acts in the usual manner, even in the usual small doses. The state of the system, which imparts this peculiarity, he considers a peculiar diathesis. It varies in force, both in different peripneumonies, and at different periods of the same case; and in general it follows the course of the disease, increasing and diminishing as it increases and diminishes, and finally disappearing with it. Hence the dose, in which the remedy must be used, is very various; and, on that account, he always commences with small doses, and goes on increasing them as long as they do not cause nausea. *But the smallest dose with which he ever commences the treatment of a case is twelve grains during the day, and as much during the night;* and if the disease has previously made some progress, he administers a scruple or half a drachm at once. As the disease goes on increasing, the doses must be augmented. The practitioner, on the other hand, must not always diminish the doses when the disease begins to abate. For the capability of enduring large doses sometimes continues, although the symptoms diminish; and in that case, if the remedy is not kept up to the full extent, the symptoms will be again aggravated. In fact, the doses must be regulated, not by the state of the disease, but by the power the system possesses of resisting them; they should not be diminished till they begin to excite nausea, or other signs of intestinal irritation. Occasionally, he remarks, the diathesis decreases, though some of the severe symptoms, particularly the dyspnœa and affections of the mind go on increasing. In such cases he uniformly found that in the lungs, organic changes were going on, which were the immediate forerunners of death. The salutary effects of tartar-emetic are not owing to its emptying the stomach, or acting as a diaphoretic, or giving a *salutary succussion* to the viscera of the chest and abdomen. It is owing to a true counter-stimulant action, (like that of digitalis), which may produce its effect in subduing the *diathesis of stimulus*, without causing any sensible evacuation. But it is a remedy much more appropriate than digitalis, which, besides being too irregular in its action, is also too dangerous in its effects, when carried farther than the inflammatory diathesis enables the system to endure it. Rasori has seen the tartar-emetic produce rapid diminution of the pulse, like digitalis; but he never observed a reduction under 50. In many cases the treatment of peripneumony may be entrusted to the tartar-emetic alone. But frequently also it is requisite to conjoin general blood-letting; for the disease may have advanced so far, or may be advancing so rapidly, as to threaten the pulmonary tissue with speedy destruction; and then it would be dangerous to wait so long as is generally required for the action of the remedy to be fairly established. Other reasons of convenience likewise render bloodletting frequently advisable where it is not absolutely necessary. At the commencement of the paper, the author remonstrates with much earnestness against the unwillingness of his countrymen and others to believe the statements which had previously gone forth into the world regarding his singular practice. "My novel and daring mode of administering tartar-emetic," says he, "so astonishes the physicians, who, for the first time, witness it in my Clinique, that they question the proper preparation of the medicine, or the punctuality of the patients in taking it. Some have removed their doubts by procuring a part of it, and subjecting it to chemical analysis, or administering it to their own patients, or taking it themselves; and others have become convinced by seeing my patients take it, or actually administering it with their own hands, and then waiting in vain for its usual emetic effects." The translator of the memoir bears witness to these facts, having himself been at one time in the ranks of the incredulous; and having become satisfied of Rasori's statements, by analyzing the drug used by him, and administering it to other patients.

On the use of Iodine in Goitre and Scrofula. (*Jour. Complément. Fevr.* 1824, xvii, 305.)—This Journal contains a long and interesting memoir on the medicinal use of iodine, by Dr J. G. Kolley, of Breslau; who completely confirms the statements of former writers as to its efficacy in some diseases, and adds some important particulars relative to the circumstances under which good effects may be anticipated from it. He always used it in the form of tincture, as first proposed by Dr Coindet. He has found that it does not agree with persons of a sensible nervous habit; in them a very few doses excite restlessness, lassitude, weight and pain in the head, tremors of the limbs, moroseness, spasms, a sense of anxiety in the chest, and invincible repugnance to continue the remedy. It likewise disagrees with very robust muscular individuals, accustomed to vigorous nourishment, especially if they are liable to determination of blood towards the head: in such persons it is apt to cause headaches so violent as almost to drive them to madness; and if employed at all, must be conjoined with laxatives. Wine-bibbers are similarly circumstanced. If there is the slightest disorder of the stomach or intestinal canal, iodine is apt to cause dyspepsia. Febrile and inflammatory diseases are quite incompatable with it; and so are diarrhœa, local urinary disorders, phthisis, all local affections of the lungs, stomach and liver, and in general all internal diseases that have a tendency to run on to suppuration. In cases of goitre its success depends on the tumour being actually situated in the thyroid gland,—on the gland being neither scirrhous nor carcinomatous, nor filled with hard concretions or other analogous productions,—on the disease being not too old,—on the absence of cachexy, hectic fever, and derangement of the *primæ viæ*,—on the goitre itself being neither inflamed nor painful. If care be taken to abstract these circumstances, Dr Kolley considers that the practitioner will very rarely fail of success. With regard to scrofula (scrofulous glands and sores, we presume; for his description is not very clear), he remarks, that iodine often shows singular activity in dissipating that disease. But to insure its success, or even its safe administration, the patient must be neither hectic, nor phthisical, nor disposed to be so. In the case of children, a steady regimen is requisite, great cleanliness must be observed, they should be kept in a pretty uniform temperature; and if they are very irritable, of a weak habit, liable to diarrhœa, or hydrocephalus, the remedy must be used with great caution, or avoided altogether. In scrofula he found it of great advantage to combine the iodine with mercury. The cases he has selected from his practice are twelve in number, five of goitre, three of scrofula, and four of eruptive diseases described under the convenient general title of *dartre*. We shall notice briefly his success in the two first diseases. The first case of goitre is a most admirable history of that disorder as it occurred in himself. It had existed for many years, had reached such a size as to impede greatly his speech and breathing, and all remedies had been directed against it in vain. He took the tincture of iodine for twelve weeks; in about a month the tumour began to swell a little and soften, then diminished, and at the end of the twelfth week had disappeared almost entirely. The second case is that of a girl 18 years old, in whom the disease had existed since her fifth year. When she had used the iodine for a month, the tumour began to diminish: after taking it for sixteen weeks, her stomach began to be affected, and consequently the remedy was abandoned: when the author saw her three months afterwards no trace of the goitre remained. The third patient, a stout man, 36 years of age, with a large goitre of twenty years standing, took the iodine for eight weeks; and the tumour disappeared entirely four weeks afterwards. In the fourth, a girl of 19, the disease was of sixteen years standing. During the third week of the treatment she was seized with a sense of anxiety, heat in the throat, and excessive faintness each time she took the iodine. In consequence no consideration could induce her to persevere, though the tumour began to decrease. Dr Kolley is ignorant of the issue in this case. The last was an incipient case, accompanied with hypochondriasis. The tumour was removed in no long time, as well as many of the sensations referred by the patient to the disease in the neck; but he continued liable to congestions in the head. Of the three scrofulous cases, one was a most deplorable example of the ravages occasioned by a scrofulous diathesis of the most exquisite kind. Among other maladies she had enlargement of the cervical glands, and suppuration of several of them, accompanied with the discharge of serous matter. All the anti-scrofulous remedies had been exhausted upon her in vain. When the iodine had been continued four weeks in the dose of five drops (tincture) thrice aday, the sores began to discharge a thicker yellow pus, some of them appeared about to cicatrize, and the swellings in the neck diminished. At this period the iodine was intermitted, calomel was given to gentle salivation, and the iodine then resumed. In eight weeks the sores were all healed, and the swellings gone; and the patient never experienced an unpleasant symptom. Ten months afterwards the cure continued permanent. The second case was one of most obstinate and complicated strumous ophthalmia in a well characterized scrofulous habit. All remedies had proved unavailing. In fifteen days there was a sensible amendment under the use of the iodine; and in three months the ophthalmia and all its effects which were curable had disappeared. The third patient, a child of four years, liable to severe strumous ophthalmia, which had resisted all sorts of treatment, got well in two months, under the combined use of iodine and mercury. In both cases of ophthalmia, Dr Kolley thought it necessary to remove the active state of the inflammation by ordinary means, before resorting to the iodine.

Extirpation of the Parotid Gland. (Arch. Gén. de Méd. Janv. 1824. iv. 60.)—Professor Beclard of Paris has at last presented the surgical world with a genuine case of the complete extirpation of the parotid gland. The subject of operation was a stout healthy man, 47 years old, affected with a cancerous tumour over the right parotid region. It extended upwards so far as to raise the lobe of the ear, and appeared to involve the cartilaginous portion of the *meatus auditorius externus:* downwards it extended an inch beyond the angle of the jaw: it adhered behind to the anterior edge of the sterno-mastoid muscle; and it covered anteriorly almost the whole of the masseter muscle. It was immoveable, and ulcerated in two places; but there was no other mark of a cancerous diathesis. The operation was performed at the patient's request. A curved incision having been made above and below, so as to include the whole tumour, M. Beclard proceeded to dissect it backwards from the masseter muscle. He then tried to detach it also from below upwards, but found this to be impossible on account of a deep prolongation of it behind and beneath the internal pterygoid muscle. This prolongation he therefore divided, while he continued to raise the tumour upwards by dividing the cellular tissue which connected it with the adjoining parts before and behind the prolongation. During this part of the dissection, the inferior semicircumference of the cartilage of the auditory passage was removed, as it was involved in the disease of the parotid gland. A part of the sterno-mastoid muscle was likewise removed for the same reason. Some bleeding vessels being then secured, and the principal part of the tumour altogether removed, the operator proceeded to cut away, by successive slices, the remaining part, which extended deep behind the jaw. At last, when it was nearly all cut away, a sudden gush of arterial blood announced the division of the external carotid, or of a large branch near its origin in that artery. The vessel was seized, however, with a pair of forceps, beneath the opening; and while the hemorrhage was thus suspended, a double ligature was passed round it, and one tied on each side of the aperture. The artery was then held forwards and outwards, till all that remained in the parotid fossa was extirpated. A small portion only of the tumour, immediately before the vertebral column, was not cut out, because of its proximity to the internal jugular vein; but a double ligature was passed under its middle, and one tied on each of its extremities. The wound now showed anteriorly the masseter muscle dissected bare, the facial artery bare, but uninjured—posteriorly the mastoid process of the temporal bone, and the sterno-mastoid muscle,—and at the bottom the styloid process, the external carotid tied, the stylo-hyoid and digastric muscles, and the remaining portion of the tumour. The parotid duct, part of the platysma myoides, and the ramifications of the seventh pair of nerves were removed with the tumour. The texture of the tumour was chiefly scirrhous, partly tuberculous, but nowhere similar to that of the healthy parotid gland.

For some days no unpleasant accident occurred. The whole right side of the face, however, including the orbicular muscle of the right eye, was paralyzed. A healthy suppuration was soon established, and uniform scarlet granulations covered the surface of the wound. But on the twelfth day the patient was seized with shivering, succeeded by fever; in the evening a rosy efflorescence occupied the right cheek; and next day a phlegmonoid erysipelas had fairly commenced over the whole right side of the neck. Local and general depletion had no power to arrest this disease, which terminated by desquamation on the face, and the formation of an extensive abscess in the neck, extending under the trapezoid muscle. Notwithstanding these complicated sequelæ, by which great emaciation and excessive debility were produced, so that his life was almost despaired of, the wound continued to look well, the bottom was partly filled up, its margin began to contract, the ligatures dropped from the carotid artery, and the isolated portion of the tumour which was left behind sloughed away. The abscess of the neck was treated in the usual mode, and with success; and the man was beginning to recover strength; but a state of delirium, which had existed from the first invasion of the erysipelas, was now succeeded by alternate fits of taciturnity, and maniacal frenzy, apparently indicating an attack of chronic meningitis. Three months after the operation the wound was entirely closed, except a small portion near the ear, which had assumed somewhat of a cancerous appearance. Three weeks af'er that the patient died. The external carotid was found obliterated, the internal jugular likewise obliterated a little farther down, the external auditory passage filled with pus, but the tympanum uninjured, the pia mater of the brain minutely injected, especially the tela choroidea, the lateral ventricles filled with serum holding in suspension a quantity of matter like wine-lees,—and there was not a vestige to be seen of the parotid gland. To this interesting case some conclusions are added, of which we shall quote those only connected with the operation. 1. The parotid gland, contrary to the opinion of some authors, is certainly liable to scirrhus. 2. The possibility of its extirpation is placed beyond the reach of doubt; the present case cannot be confounded with those in which the operators have been deceived by a tumour lying over the gland and compressing it. 3. Hemorrhage from the external carotid, in this situation, may be checked by applying the ligature; but in order to attain this end, the operator must not attempt to remove at one stroke the portion of the diseased gland, which plunges like a wedge, deep behind the jaw; for the tumour would prevent the possibility of laying hold of the artery; whereas, by cutting the tumour away by successive slices, he can easily seize it the moment it is wounded.

Surgical Prize-Questions.—The administrators of the prize-legacy of Mr John Monnikhoff at Amsterdam, have proposed the following subjects of competition.

I. On Fractures of the Patella. The objects to be attended to by the candidates are thus explained in the advertisement, which was transmitted to us for the purpose of insertion. "Since it is singular, that fractures of the patella, when produced without external violence, are always transverse; and that, while other fractured bones generally unite by callus, although their ends be not kept in contact during the cure, yet transverse fractures of the patella rarely unite in that way, but rather by the interposition of a ligamento-tendinous substance;—the candidates are requested to inquire, Whether there are any signs which precede and prognosticate fractures of the patella? Whether any bandage or other instrument is known for preventing such fractures, and what are they? Why do not transverse fractures of the patella unite by the interposition of osseous matter? And by what means may such a mode of union be promoted? What, in the absence of such means, are the methods, bandages, and instruments, invented and tried from the most ancient times, or which may still be tried, for uniting the separated ends? And what are the best means for attaining that end, without impeding the free motion of the knee-joint?"

II. On Incarcerated Hernia.—"As the operation for incarcerated hernia is often followed by fatal consequences, so that the patient may die a few days afterwards, and that, too, unexpectedly, the competitors for this prize are requested to answer the following questions.

"1. What are the symptoms which point out precisely the period when the operation should be performed, and which forbid delay?

"2. What are the causes which produce the fatal sequelæ of the operation for hernia, and what are the symptoms of these sequelæ?

"3. By what remedies, or method of cure, may they be prevented or cured?

"The administrators request, that, in investigating this subject, attention be paid to the inflammation of the peritonæum and the pseudo-membranes, which it gives rise to, and which are often followed by incarceration of the intestines,—to the stricture which is produced, not by the inguinal ring or canal, but by the neck of the hernial sac,—to the redoubling of this sac, which takes place at the junction of two inguinal hernias of the same side, or of a congenital and inguinal hernia;—and, lastly, to the adhesion or junction of the hernial sac to the intestines; and various other irregularities of the same sort, which may be viewed as the causes of the unfortunate issue of the operation."

The essays are to be written in Latin, French, German, or Dutch, and accompanied with a sealed note, containing the author's name and residence, and on the outside some designating motto. They must be sent, post free, to Professor G. Vrolik, or Dr G. H. Thyssen, at Amsterdam, before the 1st January 1826.

The prize for each will be the ordinary gold medal, or 300 Dutch florins. The essays, whether successful or not, to be the property of the Fund.

Amsterdam, 13th August 1823. (Signed) H. F. THYSSEN, Sec.

Method of Detecting Acetate of Morphia in Animals Poisoned by it. (Ann. de Chim et de Phys. Janv. 1824, xxv. 102.)—M. J. L. Lassaigne has very recently read a memoir before the French Inſtitute, announcing a method of detecting chemically the acetate of morphia in the bodies of animals poiſoned by it. He has applied it to the matters of vomiting and the contents of the ſtomach of cats which had been made to ſwallow it, in the inteſtines of various animals to which it had been adminiſtered by the rectum, and in the fluid of the ſac of the pleura in a dog, killed by the injection of it into that cavity. If the ſuſpected matter is ſolid, it is to be boiled for 10 minutes in water. The ſolution thus procured, or the fluid, if the ſuſpected matter was originally ſuch, is then to be evaporated by a very gentle heat till nearly dry. The reſidue is to be treated with alcohol, which diſſolves the acetate of morphia, and leaves moſt of the animal matters. This alcoholic ſolution being evaporated to dryneſs, the extract left is either to be ſubjected to the four teſts we ſhall preſently mention; or, which is better, it is to be diſſolved in water, the remaining animal matter thrown down by ſubacetate of lead, the exceſs of lead removed by a ſtream of ſulphuretted hydrogen, and the ſolution again evaporated ſpontaneouſly, or under the vacuum of the air-pump. If the solid matter left contains any morphia, 1. it will have a very bitter taſte; 2. when diſſolved in a ſmall quantity of water, it furniſhes a white precipitate with ammonia; 3. it diſengages acetic acid, when concentrated ſulphuric acid is poured upon it; and, 4. an orange-red colour is ſtruck by the addition of nitric acid. (Theſe teſts, we muſt remark, will not diſtinguiſh morphia from ſome of the other vegetable alkalis, ſtrychnia, &c., for example.) If the original fluid is alkaline, acetic acid muſt be added to rediſſolve the morphia, thrown down by thealkali.

M. Laſſaigne does not ſtate how ſmall a quantity may be detected by this proceſs. Yet it ſeems ſufficiently delicate. For on one occaſion he detected the poiſon in the ſtomach of a cat killed by five grains of it; and he had no difficulty in diſcovering a grain and a half diſſolved in ſix ounces of bullock's blood. This ingenious chemiſt has alſo made ſome experiments, which tend to confirm the reſults of ſome former experimenters relative to the decompoſition which vegetable poiſons undergo in the blood of living animals, and the conſequent impoſſibility of diſcovering them there by chemical means, even when they have been introduced directly into the veins but a ſhort while before. When 30 grains of the acetate of morphia were injected into the jugular vein of a horſe, blood drawn from the jugular of the oppoſite ſide ten minutes afterwards, gave ſenſible traces of the exiſtence of the poiſon. But in another experiment made in the ſame manner, none could be found an hour and a quarter after the injection. Neither could any be detected in blood drawn from a dog 12 hours after the injection of 36 grains into its crural veins. But theſe experiments are not altogether ſatisfactory. How could any chemiſt expect to find ſo ſmall a quantity as 30 grains in the blood of a horſe, which, we preſume, may be eſtimated at 120 pounds at the very leaſt? M. Laſſaigne muſt certainly have been deceived as to the reſult of the firſt experiment. Whence, too, did it happen, that the dog which was the ſubject of the laſt experiment lived ſo long after the injection into the veins of ſo large a quantity as 36 grains of this moſt active poiſon?

Poiſoning with Hydrocyanic Acid. (*Journal Complément. Fev.* 1824, xvii. 366.)—1. A hypochondriacal gentleman, 48 years old, ſwallowed at 8 in the morning two drachms of the ethereal oil of bitter almonds. A few minutes afterwards his ſervant, whom he ſent for to his bed-ſide, obſerved that his features became ſpaſmodically contracted, and his eyes fixed, ſtaring, and turned upwards. The cheſt was alſo expanded convulſively and hurriedly. A phyſician, who entered the apartment 20 minutes after the fatal draught was taken, found him already quite inſenſible, the pupils immoveable, the breathing ſlow, rare and ſtertorous, the pulſations, even of the heart, very feeble and recurring every other ſecond only, and the breath ſmelling ſtrongly of bitter almonds. Death enſued ten minutes after his arrival. The body was examined 29 hours afterwards. Although the temperature during that interval never exceeded 40° F., and the windows had been kept open, putrefaction had advanced ſo far, that the whole body was blown up with gaſes, the ſuperficial veſſels were blue and diſtended, and in many places there were bluiſh-green ſtains and phlyctenæ. Pure blood flowed from the noſe and mouth when it was ſtirred; and this, as well as the whole body, outwardly and inwardly, ſmelled ſtrongly of bitter almonds. The teeth were fixed, the lips pale, the fingers bent, and the nails blue. The ſtomach and inteſtines were red; the former contained ſix ounces of a browniſh fluid, which gave out a powerful odour of bitter almonds; and its inner ſurface was very red and checkered with bloody ſtreaks. The liver was dark and voluminous, and when cut the blood flowed from it quite fluid and of a deep violet colour. The ſpleen and the kidneys diſcharged the ſame ſort of blood, when cut into. The gall-ducts contained a violet-coloured bile. All the muſcles had a tint approaching to deep violet. The lungs were tuberculous, but otherwiſe in a natural ſtate. The heart was flaccid, diſtended with gaſes, and contained only a few drops of liquid, violet blood in its poſterior cavities. The brain was every where turgid with the ſame ſort of blood. The blood in every part of the body had the hydrocyanic odour; but moſt ſtrongly in the abdomen.

2. A young man, 20 years of age, ſwallowed a quantity of hydrocyanic acid, prepared according to Ittner's proceſs, and amounting apparently to 3½ drachms. His comrade found him in bed at eleven at night, and thinking he was aſleep, went into bed beſide him, and did not perceive he was dead till next morning. The time when he took it, and the duration of the poiſoning, are therefore unknown. The body was examined 30 hours after his death was diſcovered. It preſented very ſlight traces of incipient putrefaction; and exhaled no odour of bitter almonds. The ſkin was here and there livid. The teeth were not fixed. The penis was erected, and emiſſion had taken

place. No fluid iſſued from the mouth or noſtrils. The ſtomach and inteſtines preſented the ſame appearances as in the former caſe, and likewiſe were eaſily lacerable. The ſtomach contained a quantity of grey chymous pulp, ſmelling of hydrocyanic acid. The liver, ſpleen and kidneys, contained much fluid and violet-coloured blood; the bile had alſo a violet tint, as in the former caſe; and the colour of the muſcles was ſenſibly deeper than uſual. The lungs were violet-coloured and gorged with blood. The anterior cavities of the heart were gorged with blood, which contained a few clots; the poſterior cavities were nearly empty. The larynx, trachea and œſophagus were red, and the trachea contained much blood. When the integuments of the head were cut, fluid blood iſſued from the wounds in great quantity; the brain was every where turgid. The blood did not exhale the odour of hydrocyanic acid in any part of the body.

It is difficult to account for the diſcrepancies between theſe two caſes; and particularly for the preſence of the hydrocyanic ſmell in the one and its abſence in the other. The author of the paper attempts to account for the difference, but without ſucceſs.

Spontaneous Combustion.—We have received from Dr Klaatſch of Berlin, an account of ſome inquiries he made into the particulars of one of the caſes of Spontaneous Combuſtion, quoted from a French journal, in our Number for laſt October. Taking advantage of an opportunity of communicating with the Mayor of Beauvais, where the accident happened, Dr Klaatſch, with the laudable deſire of ſifting ſuch a wonderful caſe to the bottom, procured a copy of the report drawn up by the Officer of Police, who inveſtigated its circumſtances. The whole body, according to this report, was found totally conſumed except the head and one leg. Near the body ſtood a braſs chaffing-diſh, containing embers; and conſequently Dr Klaatſch very properly inſiſts that this caſe cannot be conſidered one of indiſputable "ſelf-burning." At the ſame time, we preſume, there can be no doubt of its being one of preternatural combuſtibility; which, we ſuggeſted, was the limit of our belief with regard to all ſuch ſtories. It appears that the ſubject of the caſe had intended to deſtroy himſelf. About a week before his death, he champed and ſwallowed three-fourths of a drinking-glaſs, but without ſuffering any harm; and on the evening before the body was found conſumed, he bade farewell to a fellow-lodger, and aſſured him they ſhould not meet again.

Hydrophobia. We had intended to have given in the present number some account of the singular observations on the seat of the poison of hydrophobia, communicated to the Physico-Medical Society of Moscow, by Dr Marochetti, but are obliged to postpone them for want of room. In the meantime we shall give an abstract of a curious Table, inserted in Hufeland's Journal for March 1824, containing a statement of the number of deaths from hydrophobia in the different departments of the Prussian monarchy. From this it appears, that the deaths in ten years amounted to 1666; or—

Years	1810.	1811.	1812.	1813.	1814.	1815.	1816.	1817.	1818.	1819.
Deaths	104	117	101	85	127	79	201	228	268	356.

It also appears, that they occurred more frequently in some provinces than in others. They were most frequent in Marienwerder 228, and Bromberg 162, then in Breslau 90, and Oppeln 53, and in Trier 46, and Aachen 58. On the contrary, not a single case occurred in Stralsund; and it was rare in Dusseldorf 5, Erfort 6, Stettin 7, Köslin 7, Magdeburg 7, Münster 7, Amsberg 7, and Berlin 4. Dr Hufeland accounts for this great diversity, by remarking, that the provinces in which it is frequent, are contiguous to forests containing wolves, as those of Poland, Russia, and the Ardennes.

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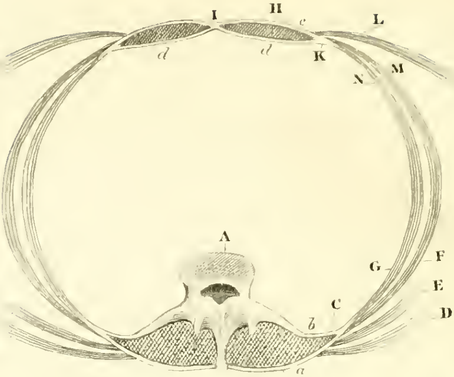

Fig . 2 .
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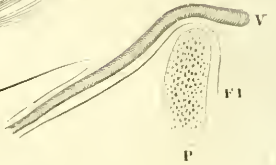

Fig . 3 . p. 302

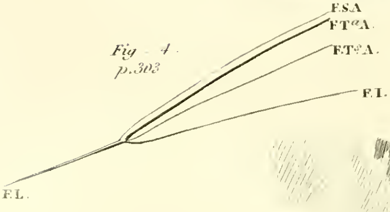

Fig . 4 .
p.303

Fig . 7 .
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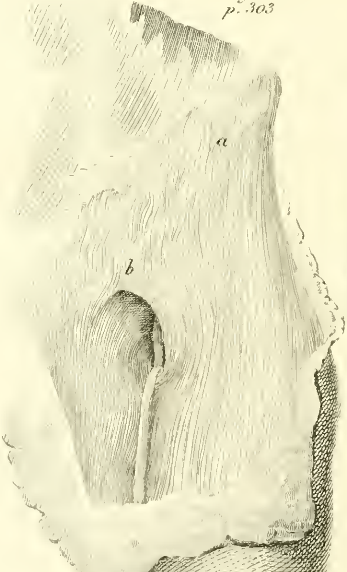

Dr Craigie's Case
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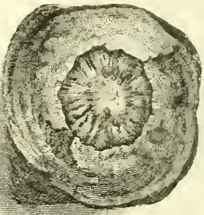

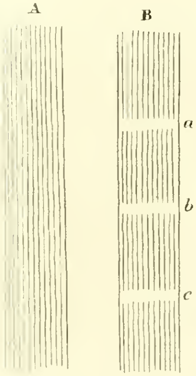

Fig . 1 .
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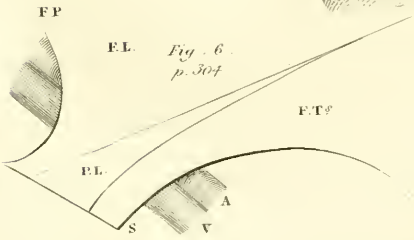

Fig . 6 .
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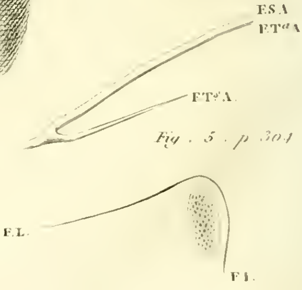

Fig . 5 . p 304

Sculpt

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MEDICAL AND SURGICAL JOURNAL.

1. OCTOBER, 1824.

PART I.

ORIGINAL COMMUNICATIONS.

I.

History of a Case, in which the Symptoms of Iliac Passion arose from the Transit of an unusually large Gall-stone, terminating favourably. By David Craigie, M. D. Edinburgh. (*With an Engraving.*)

I was requested, on the morning of Wednesday, 31st March, to see an old lady, who was reported to have been very unwell for several days with incessant vomiting, obstinate constipation, and considerable pain of the belly. I found her in a very weak and languid condition, distressed with frequent painful retching and occasional hiccup, and vomiting at intervals a yellow-coloured fluid, of the bitter taste of which she complained much. The tongue was very dry, and coated with a dark-brown crust. She complained much of thirst; and the skin was parched and imperspirable, while the face and extremities were inclined to be cold. The pulse was about 86, small, not hard, and much compressed. The whole surface of the belly was painful, so tender that she could scarcely bear the necessary examination, and there was some degree of general tension. She complained most particularly of what she called her right side; and on being requested to point out the part with her own hand, she laid it first on the right hypochondriac, and then across the lower part of the epigastric region,—as nearly as could be judged, to the site of the transverse arch of the colon.

She stated, that she had first become sick and vomited on Sunday evening (28th), after walking from Leith; that about the same time she felt considerable pain of the belly, but not limited to one place in particular, and that these complaints had become more severe on the following day, and on Tuesday, notwithstanding the employment of means to alleviate them. The bowels had not been opened since Saturday, 27th, (4 days) notwithstanding the administration of castor oil and several other medicines, most of which were rejected by vomiting. Blood had been taken from the arm yesterday without relieving permanently the pain of the belly; and injections had been repeatedly employed without moving the bowels. Careful examination of the navel, upper part of the thigh, and other situations, showed no hernial tumour; and, by her own account, she appeared never to have been subject to any complaint like rupture. I requested a young medical gentleman who was in the house, and who had already been treating her, to take from 15 to 20 ounces of blood from the arm, to administer an injection, consisting of infusion of senna and common salt, and to put her, as soon as possible, into the warm bath, at 96° Fahrenheit. A pill, consisting of equal parts of the aloetic pill and extract of hyoscyamus, was also ordered to be given every hour.

In the evening, I found her slightly relieved; the skin was warmer and more perspirable; the pulse fuller and softer, and the vomiting less frequent. The belly, however, was still painful, especially from the right hypochondriac region downwards to the hypogastric; and the bowels continued obstinately bound. About 12 ounces of blood only, which was covered with the buffy coat, had been taken, and the bath had not yet been prepared. I took 15 ounces of blood from the arm, which induced slight and transient fainting; the pills were ordered to be continued; and an opiate, containing 40 drops, by measure, of laudanum, was ordered to be given on removal from the bath.

The following morning (Thursday, 1st April), she was much worse. Had found the pain of the belly much relieved by the bath; and the medicine taken after was followed by suspension of sickness and vomiting, and sleep for some hours. But about six in the morning, the pain had returned, and continued till the present time (11 *a. m.*), when she complained much of it; the sickness and vomiting of bilious matter were almost incessant; the tongue was dry, hard, and brownish; the pulse about 88, not strong or full; and the countenance was much shrunk and pale; bowels not affected by medicine or injections. I again examined the belly, groin, and upper part of the thigh, with the utmost care, but could discover nothing like a hernial tumour. A fold of loose thickened skin, with some adipose matter in the right groin, to which she referred pain aggravated by pressure or examination, appeared at first to imitate the swelling occasioned by protruded intestine; but it was not affected by coughing; gave no sense of crepitation; nor was it in the usual site where hernia generally occurs in the female. As the symptoms of inverted peristaltic motion, however, continued urgent and unabated, and as I was unwilling to suffer a case, with all the phenomena of iliac passion, to proceed, as it seemed to be doing, to its fatal termination, without seasonably attempting every practicable and reasonable remedy, I suggested to her friends the propriety of consulting Dr Thomson, who accordingly accompanied me to see her in the course of the day.

Examination having satisfied Dr Thomson that there was no hernia, I was desirous to try the effect of the tobacco injection at once. This, however, he was disposed to defer as a last resource, and employ, in the mean time, the fetid enema, with turpentine. Two drachms of assafœtida, dissolved in the usual manner, and combined with an ounce of oil of turpentine, were injected into the colon; 18 leeches were ordered to the belly, and warm fomentations were substituted for the hot bath.

In the evening she appeared rather worse. The countenance was pale, ghastly, and shrunk; the surface and extremities were cold; the pulse was about 80, small, and very variable; the epigastric region and belly in general were very painful, and somewhat tense and tympanitic; retching and vomiting were incessant and distressing; and she complained much of the eructations of turpentine. The injection had continued in the colon four or five hours, which was longer than the previous ones; and, upon the administration of a common one, was partly returned, but without a particle of feculent matter. I ordered the fetid injection to be repeated as soon after as possible; and, as she was importunate to have the opiate from which she had experienced much relief the previous evening, this was likewise permitted to be given at bedtime.

The following morning (Friday, 2d April), though she had been easier, and had vomited less in the early part of the night, she was not essentially better. The abdominal pain was not relieved, but vomiting recurred at intervals; and the bilious matter was much darker, and seemed to be accompanied with minute portions of fæcal matter, while the bowels continued obstinately bound. It was now determined to have recourse to the tobacco injection; and after infusing one scruple of the dried leaves in eight ounces of boiling water for about half an hour, I caused it to be injected gradually, with the precaution of ascertaining that it entered the bowel, and did not produce its effects too suddenly or too violently. It was retained from twelve till four, when eight ounces of senna infusion, with four drachms of common salt, were injected, and brought some of it off. During its operation the patient was sick, languid, very faint and listless; and the pain of the belly was much diminished. The sense of internal motion of the bowels was faintly roused, and some degree of expulsive effort was felt, but no feculent stool was voided. In other respects she was the same as in the morning. After ordering the use of the hot bath at 96° for 15 or 20 minutes, according as it was borne, and an hour after removal from it, the administration of half a scruple of tobacco, which was prepared as before, I left her at seven for that evening. She then appeared in a condition so wretched, that I apprehended the notice of her death ere morning. She indeed appeared thoroughly persuaded that she was at the point of death; and resisting, so far as her extreme weakness would allow, the repetition of the injection, earnestly requested that she might be suffered to die in peace.

The following day, however (Saturday 11th, *a. m.*) we found a sensible improvement. Since the use of the bath, in which she had continued fully 20 minutes, last night, the vomiting had not returned; sickness was much diminished, she had slept a good deal, the skin was warmer and more perspirable; the pulse was about 85, fuller, and less inclined to vary; the pain of the hypochondriac region and belly, in general, was still considerable, but not so acute as during the last two days. A senna injection administered according to order this morning, came away shortly after, with some slight traces of feculent matter. It was therefore agreed to repeat the injection of tobacco infusion to-day in the same manner, and to alternate it with another consisting of two drachms of fetid gum dissolved in tepid water, with a drachm of anise-seeds reduced to powder. This day was passed in more ease. The vomiting had not recurred since last night, the belly was slightly easier, but the bowels continued bound. The infusion of half a scruple of tobacco was injected late in the evening, and the fetid injection was ordered to be repeated with half an ounce of oil of turpentine, the following morning.

Next day (Sunday 4th April) we found her continuing easier. The pain of the belly was decidedly relieved, she had retched only once last night, and had no vomiting. The tongue was dry and much furred, the skin warmer, but not particularly dry, the belly still tender, and rather tense. Very slight traces of feculent matter in one of the injections when it came away.

The usual dose of tobacco injection was immediately thrown into the intestine with the utmost care, that it should ascend into the colon. The effect of this, which proved to be the last injection requisite, was remarkable. A few minutes after its administration, the patient began to experience first a sensation of intestinal motion, then considerable sickness and distressing faintness; and, at length, in about 15 minutes, a desire for the bed-pan. After continuing on it for a few minutes, supported by the nurse in the requisite position, she voided, with a feeling of acute pain, some hardened excrement, uttered a loud shriek, and instantly fell from the nurse's arms on the bed, in a state of complete insensibility. In this condition, she continued for about ten or twelve minutes, without giving the slightest sign of consciousness; the motion of the heart continued but very feebly, respiration was extremely languid, irregular, and interrupted, and every muscle appeared to be relaxed. As this seemed more like a hysteric paroxysm than actual syncope, I was not particularly anxious to shorten it; under the use of the usual analeptic remedies, however, she gradually recovered her consciousness, but continued fully one hour in a very languid and exhausted condition.

In the mean time, on looking at the matters voided, I found she had passed a globular consistent mass fully one inch in diameter, which I ordered to be preserved till the following morning, when I should have an opportunity of examining it with Dr Thomson. From a cursory view, and the general appearance of the other matters, I thought it might prove to be an intestinal concretion, and I was desirous therefore, to examine it in the presence of another gentleman, that there might be no ground for mistake. I was at all events satisfied, that now when this, whatever it might be, was expelled, the cause of obstruction was probably removed, and the intestines would resume their usual functions. With this view, in the mean time, I ordered the nurse to administer, as soon as the state of the patient admitted, an injection consisting of infusion of senna, common salt and oil, and after its operation, that the patient should take every hour, till the bowels were fully opened, ounce doses of a solution of Epsom salts, prepared according to a suitable formula.

The following morning (Monday 5th April) we found that the bowels had been freely opened, and had discharged a considerable quantity of fluid feculent matter, but no more solid masses. In other respects she was quite relieved, but still complained of pain and weight of the right hypochondriac region, and tenderness across the epigastric. I may add, in this place, that though these symptoms continued in various form and degree for many days, and feverish attacks recurred at intervals, by proper management they have entirely disappeared, and the individual is at present in good health.

Having procured a knife for the purpose of dividing the globular mass, I proceeded to examine the nature of the substances first discharged. The moment I removed it from the clear water in which it had been immersed, Dr Thomson exclaimed that it was a gall-stone; and this opinion was immediately confirmed by the section which was now made. It proved to be a biliary concretion, with the usual appearance, of a figure nearly spherical, weighing 160 grains, and 1 inch 2 lines in diameter. The central section showed it to consist of the following parts. 1*st*, A globular mass of highly crystallized matter, about 5 lines in diameter, of a brown or chestnut-brown colour, a resinous lustre, and uneven crystalline fracture. This central portion resembles a fragment of the mineral called *resinous grenatite*, or *colophonite*, more closely than any thing else to which I could compare it. 2*d*, Of a concentric spherical shell two lines thick, of a lighter colour verging to yellow, and of less lustre and less crystalline than the central nucleus. 3*d*, Of another concentric spherical shell, less perfectly formed, or thicker at some parts than at others, and giving the section an angular appearance. This outer portion was of a dirty brown colour, less crystalline than the inner, and in appearance and consistence not unlike unburnt cork. These were its characters for several days after it was discharged, but it has now undergone some changes. It is much lighter, and the half which I retain weighs only 63 grains. It is friable, and the greatest care is requisite to preserve it from being broken into fragments. The colour of the central *nucleus* is much less deep, and, when examined attentively, is found to be chiefly white, or colourless plates mixed with orange, or orange-brown matter interposed. For the following notice of its physical and chemical characters, I am indebted to Dr Turner.

" The nucleus is composed of a crystalline, colourless, transparent substance of considerable lustre, which has a lamellated arrangement, and, like mica, may be separated by a knife into fine lamellæ. An orange-yellow matter is deposited partially, and in small quantity, between its layers, which prevents the transparency and lustre of the crystalline mass from being so considerable as it otherwise would be. The central part is surrounded by a circular shell, in which the same substances are visible, though they are less regularly disposed, and more intimately mixed than in the nucleus. This shell is enveloped by an outer crust, which is of a deep brown colour, and has no crystalline appearance; but, on examining it with a magnifier, small brilliant micaceous points were very distinctly perceived, "These three parts of the calculus are composed of the same principles in different proportions. The first or nucleus contains a very minute quantity of colouring matter, the rest being the crystalline substance. The middle shell is analogous to the preceding, except that its two constituents are more intimately mixed. The outer crust contains a considerable share of the crystalline substance, but which is concealed by a larger proportion of colouring matter.

"The crystalline matter fuses when held near the flame of a lamp, and concretes again on cooling. It is insoluble in boiling water, in solution of potash, or in dilute acids. On the contrary, it dissolves readily in boiling alcohol, and crystallizes, on cooling, in the form of delicate scales of a brilliant lustre. It is the substance of which nearly all biliary calculi in the human subject are composed, and which Fourcroy called *adipocire*, because he considered it identical with spermaceti, and the fatty matter obtained from muscle. M. Chevreul maintains, in a recent work, that Fourcroy, and subsequently Thenard, both erred in this respect; and contends, that the crystalline matter of biliary calculi is quite a distinct substance, which he terms *cholesterine.* My observation coincides with that of M. Chevreul, for I find a striking difference between spermaceti and the crystalline calculous matter. They both fuse at a low temperature, but the spermaceti much more easily than cholesterine. This is shown by putting them into a flask of cold water, and applying heat; the former fuses considerably below the boiling point, and swims like oil upon the surface; the latter continues solid during the most violent ebullition. I boiled the cholesterine in solution of pure potash for a considerable time without any change; the spermaceti, on the contrary, is readily acted upon under the same circumstances.

"The colouring matter of the calculus, though differing in its shade at different parts, from greater or less concentration, is identical throughout; it gives the same coloured streak on paper, and has the same chemical properties. It is insoluble both in hot and cold water, in boiling alcohol, except in extremely minute quantity, which is deposited on cooling, and in dilute acids. It dissolves readily in solution of pure potash, and forms a deep greenish-brown solution, which is yellow when diluted. It is precipitated from its solution of a green colour by acids.

"It hence agrees exactly with the properties said by Thenard, in the 1st volume of the Mem. D'Arcueil, to characterize the colouring matter of the bile.

"It appears, then, that the calculus under examination is composed of cholesterine and the colouring matter of the bile; and from the properties of these substances, it will be obvious that alcohol affords a ready means of separating them.

"I examined all the different parts of the calculus for the presence of inspissated bile, by digestion in water. The clear liquid, after decantation, had no taste, and no alkaline reaction; nor did sulphuric acid or acetate of lead cause a precipitate. No inspissated bile, therefore, was contained in the calculus."

This case presents three peculiarities, which deserve the attention of the pathologist.

1*st*, It is at once interesting and uncommon, to find a gall-stone so large, descending through the ducts by any means, but especially with so little permanent injury to them as it appears to have caused. It is not extraordinary to find biliary concretions of various size, in some instances pretty large ones, in the gall-bladder after death; and the writings of physicians and anatomists contain several examples in which dissection disclosed concretions as large, or even larger than that of the present case, and which may have remained in the gall-bladder perhaps for years.[*] It is, however, less frequent to see large concretions expelled from the living body by the natural passages; and we find a few examples in which this had taken place, recorded as curious facts by medical authors, such as Hooke, Bianchi, Biondi, Meyer and Lettsom.

Mr J. Yonge, in a letter to Hooke, informs him, that "he had lately seen a gentlewoman, almost dead in jaundice, relieved by the evacuation of one almost as big as a pullet's egg, and another from a man, as big as a nutmeg;—both followed with a *lask* (looseness), discharging prodigious quantities of choler."[†] John Baptist Bianchi relates the case of a lady of rank, who had been subject to periodical jaundice for twelve or fifteen days at a time, and from whom a gall-stone, larger than a walnut, was brought away by the operation of a strong purge.[‡] Biondi records the case of a woman in which a concretion of unusual magnitude had been rejected by vomiting.

In these cases, however, little attention was paid to the exact dimensions, which is a matter of some consequence, and of considerable interest to the practical physician, to whom it is important to know, what degree of distension the cystic and common ducts may undergo without permanent or irreparable injury. In this respect, the first example, perhaps, which merits much attention, is one recorded by Dr Lettsom, in the 1st volume of the Memoirs of the Medical Society of London.* A military gentleman of Jamaica had laboured for years under severe pain of the epigastric region, which was ascribed to gout. As he had intervals of ease for eight or ten days, Dr Lettsom suggested that his complaints depended on the presence of gall-stones. His sufferings were somewhat mitigated by the warm-bath and liberal doses of laudanum; and during one of the fits, while castor oil and opiates were exhibited by the mouth, and purgative and anodyne glysters were administered by the intestines, after four days struggling, he at length voided a firm hard concretion, 2¼ inches long, of an oblong spheroidal figure, with a slight contraction in the middle, and weighing 1 ounce 2 drachms and 23 grains. The patient, though distressed for several days with painful spasms, at length recovered gradually. The concretion was not divided or examined chemically; and it is merely the assertion of Dr Lettsom that is our evidence for believing it a gall-stone. If it was so, I am inclined to regard it as the largest known to have been discharged during the life of the individual.

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[*] See among others those mentioned by Betzold apud Halleri, Dissert. tom iii. Coiterus, Schaarschmidt, Schurig, Robinson, Heister, &c. and several delineated or mentioned by Sandifort in his Museum Anatomicum. Leidae, 1793.

[†] Philosophical Experiments and Observations of the late eminent Dr Robert Hooke, F. R. S., &c. and the other eminent virtuosos of his time. Published by W. Derham, F. R. S. Lond. 1726, p. 79.

[‡] Historia Hepatica Joannis Baptistae Bianchini, M. D., &c. tom. i. p. 189.

The next case of importance is related by Mr H. L. Thomas, in the 6th volume of the Medico-Chirurgical Transactions; and it is interesting chiefly in the resemblance which exists between the symptoms to which it gave rise, and those of the case which I have described above. The patient, a woman of 63, laboured under the usual symptoms of obstructed bowels, so distinctly like those of *ileus*, that the physician in attendance, on finding the intestines obstinately locked against all remedies, and an irreducible umbilical hernia, directed the employment of surgical aid. Mr Thomas, who was summoned with this intention, convinced, from the history and symptoms of the case, that there was no chance of effecting reduction, proposed the operation; but, as the minds of the patient and her friends were not prepared, it was deferred till the following morning. In the mean time, a bolus, containing 10 grains of calomel, and an opiate glyster, were ordered to be administered; the belly was to be fomented with a warm mixture of vinegar and brandy, and a spoonful or two of cold water was directed to be given occasionally. The night was passed comfortably; and, on the following morning, the vomiting had ceased; the skin had become warm and moist, the thirst less intense, and the pulse calmer. During that day and the subsequent night, the symptoms, though milder, varied much; but about nine the next morning, after much suffering, an evacuation suddenly took place, which was found to contain an oval biliary concretion 228 grains in weight, 1.6 inch longest diameter, and 1.1 inch smaller diameter. The analysis of Dr Marcet showed it to consist chiefly of adipocire, tinged with vitiated bile. The individual survived in this case;—and, though the point was not examined by dissection, Mr Thomas thinks there is reason to believe that the concretion had descended along the ducts, and had not been attended with any permanent injury by ulceration. *

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* Vol. I. p. 575. Case of a Biliary Calculus, by J. C. Lettsom, M. D. &c.

The Second Part of the 12th volume of the Medico-Chirurgical Transactions, published last season, contains two interesting cases of gall-stone, expelled during the life of the individuals, by Mr T. Brayne of Banbury. In the first case, the patient, a woman of 55, was liable to fits of pain in the epigastric region, which returned every two or three weeks for five or six successive times, and were generally, but not always, succeeded by slight jaundice, which soon disappeared, and left her as well as usual. In November 1820, she became affected by symptoms of continued fever, but without local pain,—till October 1822, when severe pain of the left iliac region, accompanied by tenderness on pressure, supervened. After this symptom had lasted for sixteen or eighteen hours, she became suddenly easy, and passed a natural alvine evacuation, which contained a concretion of the shape of a pigeon's egg, $1\frac{5}{8}$ inch greatest diameter, $1\frac{1}{8}$ shortest diameter, and weighing 162 grains. It consisted of adipocire or cholesterine, which, from the description and delineation, appears to have been crystallized exactly in the manner in which that of the central nucleus of the one described above was disposed. The patient lived one year and a half after, and died in March 1822, with symptoms of hydrothorax. It appears, from the dissection given by Mr Brayne, that "the cystic and hepatic ducts were of the usual dimensions, but that the gall-bladder was smaller, and very much thickened, and had formed a strong adhesion, about the size of a shilling, to the duodenum, close to the pylorus. There was no uncommon appearance of vascularity; but a communicating aperture, large enough to admit a crow-quill, was discovered in the centre of the adhesion." Mr Brayne thinks "there can be no doubt that the aperture in the adhesion was once large enough to give passage to the stone." †

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* Medico-Chirurgical Transactions, Vol. VI. p. 99. Case of Obstruction of the large Intestines, occasioned by a Biliary Calculus of extraordinary size, by H. L. Thomas, Esq.

† Vol. XII. Part 2. An Account of two Cases of Biliary Calculi of extraordinary dimensions. By T. Brayne, Esq. of Banbury, p. 225.

The second case related by Mr Brayne, occurred in the person of a married woman of 65, and resembled the case described above and that of Mr Thomas, in presenting the symptoms of iliac passion. On the 24th of February 1822, when she was first seen, she had laboured under complete obstruction of the bowels since the 19th, that is for five days. She was lying on her back, with her knees raised, the belly prodigiously inflated, the extremities cold and damp, and the pulse scarcely perceptible at the wrist. She vomited incessantly, and had frequent griping pains. She was ordered pills of croton oil, warmth to the extremities, hot fomentations to the belly, and, after an hour or two, a little wine,—the first of which was vomited. After some hours, the extremities had become warmer, and the following morning the stomach was more tranquil; but the bowels continued obstinately bound. The remedies of this day were castor oil, and, at proper intervals, the colocynth pill, with minute doses of opium. In the evening, some thin dark-coloured stools were procured, and, after some days, by the use of purgatives and injections, natural evacuations were passed, while all the unfavourable symptoms gradually subsided. On the 11th March, after griping, tenesmus, and straining for two days, a flat cubical concretion, with rounded angles and concave or depressed sides, and weighing 176 grains, was passed. Its greatest length appears from the engraving to be a little more than one inch. On the 17th March, after the same symptoms, she voided a smaller concretion of a hemispherical shape, or like half a large pigeon's egg, and weighing 159 grains. The individual was living at the time the account was communicated, and no dissection is given.

The case which I have described above, is the next of this description; and it is a peculiar circumstance in its progress,—that a gall-stone so large must have descended through the cystic and common ducts into the duodenum,—that it was voided of its present size,—that this process was accomplished, as there is reason to believe, without permanent injury to the ducts, and that the patient is still living, and in excellent health.

2d, The second peculiarity in the present case is, that the skin of the individual never became yellow in the whole course of the disease. It is regarded as an established doctrine, that one of the most common and certain symptoms of the presence of a concretion or concretions in the ducts, is yellowness of the skin; and many instances of jaundice are ascribed to the obstruction arising from the delay of a concretion during its transit from the gall-bladder to the duodenum. The truth of this is inculcated, not only by systematic writers as Sauvages, Cullen, and Baillie, but by authors of monographical treatises from Coe down to Saunders. Yet it is remarkable, in the present case, that neither before the appearance of the symptoms of iliac passion, nor after their accession, was the slightest tinge of yellow observed, though the unusual size of the concretion would have led us to regard this as a very likely effect of its presence in the ducts. This was indeed the reason that I never suspected the presence of a gall-stone, and ascribed the symptoms, when no hernial tumour could be detected, either to insuperable obstruction of the transverse arch of the colon, or to an internal strangulation in some part of the intestinal tube.

3d, A third peculiarity of the present case, not less worthy of attention, is, that the concretion, in passing through the cystic and common ducts, gave rise to all the symptoms of Iliac Passion. I regard it as quite certain, that the gall-stone must have been lodged in the gall-bladder previous to the accession of the iliac symptoms. If it had been in the intestine it must have been propelled downwards by the peristaltic motion, and voided as any other foreign body; and it is impossible to conceive that its passage through the duodenum and ilium could have given rise to the acute abdominal pain, the vomiting and obstinate constipation, in short to inversion of the peristaltic motion of the intestines, unless we admit that some part of the tube must have been unusually contracted. Of this it is almost unnecessary to say we have no evidence; and there is not a single point of the whole track of the tube at which we could suppose a concretion, even of the present size, to be retarded after it had entered once the duodenum. The iliac symptoms appeared first on Sunday evening, and continued till the following Friday, when they subsided, and never recurred. Now, if we admit that, previous to their accession on Sunday, the concretion was in the gall-bladder, there is the strongest reason for thinking that the iliac symptoms took place soon after the concretion had fairly entered the cystic duct, and that their continuance till Friday evening corresponded with considerable accuracy to the time which the concretion took to traverse the cystic and common duct, and reach the duodenum, when these symptoms subsided. The interval between the disappearance of these iliac symptoms and the final expulsion of the calculus on Sunday, appears to denote the time which it took to traverse the ilium and colon. I admit that some of these conclusions are merely probable conjectures; but they correspond so accurately with the progressive changes in the symptoms, that I am inclined to give them more weight, than, under other circumstances, they would merit.

A single observation on the doses of the tobacco injection, shall conclude these notices. A scruple of the dried leaves was the quantity used each time; and this was thought the safest method of administering a medicine, the operation of which is irregular, and sometimes difficult to manage. I have seen the common dose, a drachm, give rise to deadly sickness, fainting, convulsions, intermitting pulse, vertigo, double vision, and every thing alarming,—short of actual death; and the same quantity is sometimes injected without any inconvenience. This want of uniformity, I am satisfied, does not depend in every case on the mere qualities of the herb. It appears to bear an intimate relation to the state of the mucous surface of the intestines, particularly its absorbing power. If the colon has been recently evacuated, if it is rather empty, or if, in short, there is reason to believe that its absorbent power is at the moment considerable, a small dose is not only sufficient to produce the effect, but is even indispensably necessary to the safe operation of the remedy.

August 10*th*, 1824.

II.

Observations on Extirpation of the Ovaria, with Cases. By JOHN LIZARS, F. R. S. E., F. R. C. S. E., and Lecturer on Anatomy and Physiology, Edinburgh.

> " Cet exemple, et celui de l'amputation totale de l'utérus et du vagin, pratiquée avec succès, autorisent egalement à assurer qu'avec les connoissances profondes de l'anatomie, il n'est guère d'organes sur lesquels on ne puisse exercer avec avantage les diverses operations de la chirurgie. "—L'AUMONIER.

FROM the records of medicine, the ovaria seem as subject to disease as any other organ in the body; and, from their attaining enormous size, producing great pain, and destroying the life of the sufferer, they have early called the attention of practitioners. They appear subject to dropsy, the fluid being contained either in one or more cysts; and to dropsy, combined with various degenerations of texture and morbid productions. Various modes of treatment have been invented, and had recourse to, for the numerous varieties of this disease. For the simple dropsical affection, tapping, or *paracentesis abdominis*, has been employed; but it has only proved a temporary palliative. Puncturing, and keeping the orifice open, so as to seize hold of the sac and remove it gradually, has effected a permanent cure; and the same end, it will appear, has been accomplished by the operation of gastrotomy, or the making a free incision into the abdomen, and removing the entire sac, especially where the tumour has consisted of some firm substance.

Le Dran cured dropsy, with scirrhus of the ovarium, by incision and suppuration; and Professor Dzondi of Hallé informed me that he had frequently cured this disease by incision, and the introduction of a tent, and afterwards removing the sloughing sac by the forceps.

But the total extirpation of the ovarium, both in a healthy and diseased state, has been performed; and it is the design of this paper to consider the propriety of such an operation, with the view of obtaining a radical cure of dropsical ovarium. "On châtre," says Morand, "les femelles non seulement des volatiles, mais même des quadrupeds, sans danger. Cette operation appliquée aux femmes n'a point paru une chimere à Felix Platerus et à Diemerbroeck; c'etoit au rapport de Heyschius une operation commune chez les Lydiens, pour des raisons qui ne sont point de l'art." In other examples, also, the peritoneal cavity has been freely laid open, both accidentally and intentionally, and the intestines exposed to the contact of the air, without injury. Paulus Barbette of Amsterdam laid open the abdomen, and disengaged the strangulated or twisted intestine in a case of volvulus. Bonetus relates the case of a lady who was dying of intusasception, when a military surgeon opened the abdomen, disentangled the twisted intestine, and effected a cure. Schacht operated for the same peculiar disease with success. Besides these, there are many well authenticated cases of the Cæsarian operation performed, even for six times on the same woman, with success. In mostly all the cases of diseased ovaria on record, and in all the dissections of this disease which I have witnessed, the tumour was appended by a small pedicle, merely the broad ligament of the uterus. The largest I ever saw, I have kept as a preparation; and although a great quantity of the gelatinous matter, and all the serous fluid, has been necessarily removed, it still weighs 25 libs.

But the practicability of extirpating a diseased ovarium does not rest on theory. It has been proved by experience. L'Aumonier, who was chief surgeon of the great hospital at Rouen about fifty years ago, extirpated the ovarium successfully; and since his time an ovarium has been repeatedly removed, and sometimes with success, particularly in France, Germany, and America. Dr Smith of Connecticut lately extirpated an ovarium in a dropsical state, successfully. Three very instructive cases occurred to Dr Macdowal of Kentucky; and the following history of them was sent, about seven years ago, to the late celebrated surgeon Mr John Bell, who was then on the Continent, and came into my hands as having the charge of his patients and professional correspondence during his absence.

"In December 1809, Dr Macdowal was called to see Mrs Crawford, who had for several months thought herself pregnant. She was afflicted with pains similar to labour pains, from which she could find no relief; and so strong was the presumption of her being in the last stage of pregnancy, that two physicians, who were consulted on her case, requested," says Dr Macdowal, "my aid in delivering her. The abdomen was considerably enlarged, and had the appearance of pregnancy, though the inclination of the tumour was to one side, admitting of an easy removal to the other. Upon examination, I found nothing in the uterus; which induced the conclusion that it must be an enlarged ovarium. Having never seen so large a substance extracted, or heard of success attending an operation such as this, I gave to the unhappy woman information of her dangerous situation. She seemed willing to undergo an experiment, which I promised to perform if she would come to Danville, the town where I resided, a distance of sixty miles. This seemed almost impracticable, by even the most favourable conveyance; yet she performed the journey in a few days on horseback. With the assistance," says Dr Macdowal, "of my nephew and colleague, I commenced the operation, which was conducted in the following manner. Having placed her on a table of the ordinary height, on her back, and removed all her dress which might in any way impede the operation, I made an incision on the left side, about three inches distant from the *musculus rectus abdominis*, continuing it nine inches in length, parallel with the fibres of the above named muscle, extending into the cavity of the abdomen, the parietes of which were a good deal contused, which we ascribed to the resting of the tumour on the horns of the saddle during the journey. The tumour then appeared full in view, but it was so large that we could not remove it entire. We put a strong ligature round the fallopian tube, near the uterus, then cut open the tumour, which was the ovarium and fimbriated parts of the fallopian tube very much enlarged. We took out fifteen pounds of a dirty gelatinous-looking substance; after which we cut through the fallopian tube, and extracted the sac, which weighed seven pounds and a half. We then turned her on her left side, so as to permit the blood to escape. After this we closed the external opening with the interrupted suture, leaving out, at the lower end of the incision, the ligature that surrounded the fallopian tube. Between every two stitches we put a strip of adhesive plaster, which, by keeping the parts in contact, hastened the healing of the incision. We then applied the usual dressing, put her to bed, and prescribed a strict observance of the antiphlogistic regimen. As soon as the external opening was made, the intestines rushed out upon the table; and so completely was the abdomen filled by the tumour, that they could not be replaced during the operation, which lasted about twenty-five minutes. In five days I visited her, and, much to my astonishment, found her engaged in making up her bed. I enjoined her to take particular caution for the future; and in thirty-five days she returned home in good health, which she continues to enjoy."

"Since the above case," says Dr Macdowal, "I was called to a Negro woman, who had a very painful hard tumour in the abdomen. I gave her mercury for three or four weeks, with some abatement of pain; but she was still unable to perform her usual duties. As the tumour was fixed and immoveable, I did not advise an operation; though from the earnest solicitation of her master, and her own distressed condition, I agreed to the experiment. I placed her on a table, laid the abdomen open, as in the preceding case, inserted my hand, and found the ovaria very much enlarged, painful to the touch, and firmly adhering to the *vesica urinaria* and *fundus uteri*. To extract this, I thought, would be instantly fatal; but, by way of experiment, I plunged the scalpel into the diseased part, when the same gelatinous substance, as in the above case, with a profusion of blood, rushed to the external opening, which I conveyed off by placing my hand under the tumour, suffering the discharge to run over it. Notwithstanding my great care, a quart or more of blood escaped into the abdomen; and, after the hemorrhage ceased, I removed, as cleanly as possible, the blood, in which the bowels were completely enveloped. Though I considered the case as nearly hopeless, I advised the same dressings and the same regimen as in the above case. She has entirely recovered from all pain, and pursues her ordinary occupations.

In May 1816 "a Negro woman was brought to me," says Dr Macdowal, "from a distance, in whom I found the ovaria much enlarged, and, as the tumour could be easily removed from side to side, I advised the extraction of it. As the tumour adhered to the left side, I changed my place of opening to the linea alba. I began the incision, in company with my partner and colleague, Dr Coffee, half an inch below the umbilicus, and extended it to within an inch of the os pubis. I then put a ligature round the fallopian tube, and endeavoured to turn out the tumour, but could not. I therefore extended the incision upwards two inches above the umbilicus, turned out a scirrhous ovarium weighing six pounds; and cut it off close to the ligature formerly put round the fallopian tube. I now closed the external opening, as in the former case, and, as the patient complained of cold and chilliness, I put her to bed before dressing her, gave her a wine glassful of cherry brandy and thirty drops of laudanum, which, soon restoring her accustomed warmth, she was dressed as usual. She was well in two weeks, though the ligature could not be removed for five weeks, at the end of which time the cord was taken away; and she now officiates as cook to a large family, without complaint."

In the year 1821, I was requested by my friend, Dr Campbell, lecturer on midwifery, to visit a woman with an abdomen as large as if in the ninth month of gestation. On examination, the tumour occupied the whole abdominal cavity, and appeared to roll from side to side; the uterus *per vaginam* felt natural, and her catamenia had been regular, but caused excruciating pain when they occurred. She stated that she was twenty-seven years of age, had born only one child, and in twelve months afterwards had a miscarriage, two or three months after which, towards the end of 1815, she became sensible of a considerable enlargement of her belly, that began on the left side, and which she attributed to several blows and kicks received from a brutal husband, from whom she was now separated; that her neighbours now abused her, and made such complaints to her employers that they dismissed her. At that time she earned, and now earns, her livelihood by binding shoes. Being now without the means of support, she applied to a county hospital, but was in a few days dismissed, on the supposition of being with child. She then consulted a number of respectable practitioners, but all of them cruelly taunted her with being pregnant. At the end of two years she perceived a small moveable swelling in her left groin, which she allowed to increase for twelve months, when she came to Edinburgh, and, on consulting a surgeon, he opened it with a lancet, and discharged a large quantity of thin matter. On examination this was a lumbar abscess, which she ascribed to a fall on her back three years previously. The evacuation of this fluid did not in the least diminish the magnitude of the abdomen; and she imagined she could distinguish between the pain of the lumbar abscess and that of the tumour in the abdomen. She was admitted into the hospital of this place, and remained for thirteen weeks, without receiving any relief. She consulted the chief medical gentlemen of this city, many of whom pronounced her pregnant, and all of them tried to dissuade her from an operation. Two put her under different courses of mercury, and, after a consultation, one punctured the abdomen for dropsy of the ovarium.

Before having recourse to the operation of gastrotomy, I deemed it my duty to have the opinion of the principal practitioners of this city, either by personal consultation, or by sending the patient to them. The woman herself also had previously waited on many of them. Some said that to operate would be rash; others, that I would kill my patient. It was agreed by all, that there was a disease of one or both ovaries; and she had been twice tapped for dropsy of the left ovary, the result of a formal consultation of some of the ablest medical men of this city. Convinced, from the history of the disease in the records of medicine, and from gastrotomy having been successfully performed for volvulus, and from the Cæsarian section, that there was little to apprehend either from loss of blood or peritoneal inflammation, I felt desirous to endeavour to relieve the woman by an operation; but was anxious to have the sanction of some other surgeon or physician besides my friend Dr Campbell, who at once offered to assist me. All whom I took to see the patient, and all to whom I sent her, said that the disease was an affection of the ovarium, but all of them condemned an operation. My patient, therefore, abandoned to her gloomy condition, called on me repeatedly, urging me to try the operation, otherwise she would do it herself. At last, as her pain became perfectly intolerable, and she was still urgent, I resolved to operate. During the preceding period I had directed my attention to the lumbar abscess, and applied caustic eschar after eschar. Wednesday, 24th October 1823, was the day appointed for the operation; therefore, on the day preceding, she took a dose of the compound powder of jalap, which operated also on Wednesday morning, so as to preclude the necessity of administering an enema; she also made water immediately before, in order to empty the bladder. The emptying of the rectum by a glyster, and the drawing off the urine, or taking care that the patient makes water, are circumstances of some consequence to be attended to, in all operations of the abdominal cavity. As inflammation appears to be induced generally by exposure to cold; and as these cases succeeded so well in America, I desired the room to be heated to 80° of Fahrenheit. When the temperature of the room had arrived at this heat, I placed the patient on a table, covered with a mattrass, and two pillows supporting her head, and commenced the operation, in the presence of Dr Campbell, Dr Vallange, late surgeon of the 33d regiment, Mr Bourchier, surgeon of the

36th regiment, and several other medical gentlemen, by making a longitudinal incision, parallel with, and on the left side of the linea alba, about two inches from the ensiform cartilage, to the crista of the os pubis, through the skin and cellular substance, when the peritoneum appeared, the recti muscles being separated by the distension consequent on the present disease and former pregnancy. I then made a small incision through the peritoneum, introduced a straight probe-pointed bistoury, and made a more extensive opening, into which I inserted the fore and middle fingers of the left hand, so as to direct the instrument, and to protect the viscera. With this instrument I made the internal to correspond with the external incision, while my friend, Dr Campbell, who assisted me, endeavoured, but in vain, to confine the intestines within the abdominal parietes. Apprehensive of peritoneal inflammation, of which many said my patient would die, I enveloped the intestines in a towel dipped in water about 98°. I now proceeded to examine the state of the tumour, when, to my astonishment, I could find none. I next requested Drs Campbell, Vallange, and Bourchier, to make themselves satisfied that there was no tumour, when Dr Vallange observed that he felt a tumefaction on the left side of the pelvis. This, on investigation, was found to be a flattened tumour of no great magnitude, at the left sacro-iliac synchondrosis of the pelvis, lying beneath the division of the common iliac artery, into its external and internal branches. Having satisfied all present that this was not the tumour which was anticipated,—that it was impracticable to extirpate it,—and that the uterus and ovaria were perfectly sound and healthy, I proceeded to return the intestines, and to stitch up the wound, carrying the needle as deep as possible, and applying straps of adhesive plaster between the stitches. Compresses of lint were next laid along, and the nine-tailed bandage bound round the body. I then carried her to bed, and gave her an anodyne draught of 40 drops of laudanum, which was almost immediately rejected. Ordered her warm toast-water and tea.

When the intestines protruded, and baffled all the efforts of Dr Campbell and the other gentlemen to confine them, I shall never forget the countenances of my pupils and the younger members of the profession. This fact of the intestines being forced out, proves, along with others, that the lungs can be expanded although atmospheric air be admitted into the abdominal cavity; the diaphragm acted with great vigour and with powerful impetuosity. The operation was performed at one o'clock of the day, and by seven in the evening she had vomited twice; had flying pains in the abdomen, a little hurried breathing, pulse at 100, and some thirst; she also felt uneasiness from inability to void her urine, which was drawn off by the catheter; and, as a precaution, I bled her to syncope, which occurred when 11 ounces were abstracted. She lost little or no blood during the operation. An anodyne draught was given her, which was again vomited. Thursday morning, she had little or no sleep, still flying pains about the abdomen, particularly in the wound, with hurried breathing, and the pulse at the same rate; the skin felt hot, and the tongue was white and a little crusted, so that I repeated the bleeding to syncope, which occurred when 13 ounces were withdrawn. After the bleeding she felt easier, and by the evening these symptoms had disappeared; I ordered her five drops of the sedative solution of opium, which remained on the stomach, but produced no sleep: I allowed only toast-water, tea, coffee, and gruels, warm. On Friday morning she felt much better; was pained only once in the hour or so, her breathing was natural, her pulse 90, and soft, her skin cool and soft, and her tongue white and moist. The urine still required the employment of the catheter. The same low diet continued. At bedtime the sedative solution was increased to seven drops. Saturday morning, had a tolerable night, and felt considerably better; felt, however, a little uneasiness in the wound, which had not troubled her since Thursday morning; her pulse was 85, and soft, the skin natural, and tongue cleaner. Felt a little appetite, and took some ground rice with sugar. To-day I dressed the wound, and found the line of incision united from the one end nearly to the other; at the pubes there was a small portion everted; the adhesive straps were renewed, but the stitches were allowed to remain. She was allowed Panada, rice-pudding, or oatmeal porridge. At eight in the evening, she felt acute pain in the right iliac region, darting upwards; her pulse was 108, full, and strong; the skin hot, and some thirst. I therefore bled her to fainting, which followed after 16 ounces were abstracted. In an hour after a domestic enema was administered, and, lastly, the sedative solution of opium. The enema operated well, and she fell asleep. Sunday morning, after a good night, she felt greatly better; no pain of wound or abdomen, no thirst, and her pulse 90, and soft, her skin cool, and tongue much cleaner. The wound was dressed, and two stitches withdrawn. She was able this morning to make water naturally; in the evening she became uneasy, the enema was repeated, and the opiate omitted. Monday morning.—Had rested indifferently, and her pulse was 100, and feeble; skin rather hot, but tongue cleaner. Pressure on the abdomen gave no pain. The wound was dressed, and all the stitches were withdrawn. An enema of castor oil was administered. Desired to have the oatmeal gruel acidulated with the supertartrate of posass. At 3 *p. m.* the enema had not operated, so that she was ordered two drachms of supertartrate of potass mixed with treacle, every two hours, till it should operate. By 8 *p. m.* the enema had operated, and brought away some feces, which gave her great relief. The pulse was 112, her skin and tongue natural, and quite free from pain. The supertartrate of potass continued. Tuesday morning, although she had slept well, and the physic had operated twice in the morning, the tongue and skin natural, and was perfectly free from pain, yet the pulse was still 112. The wound dressed; little or no discharge, and chiefly from the everted edge at the pubes. Ordered veal broth for dinner. Wednesday, eight days from the operation, had slept soundly; was free from pain; tongue and skin natural, and pulse down to 96, and soft. Ordered veal or chicken broth. She continued daily to recover from this day to Sunday, when, although the bowels had been carefully attended to, both by laxatives and enemata, yet they felt so distended as to excite much uneasiness and irritation. An enema was administered in the morning, and she took five grains of calomel, and in two hours after, half an ounce of the phosphate of soda; both of which producing no effect, the latter was repeated after two hours had elapsed. In the evening another enema was administered, which, as in the morning, brought away a considerable quantity of feces, but without relief. Two aloetic pills were therefore given every three hours, till six were swallowed, when no motion having been produced, a drop of the oil of croton was given, which in half an hour excited vomiting. One cathartic enema after another was given, till a profuse quantity of feculent matter was discharged, and then she felt relieved. From this day she gradually recovered, without any untoward symptom; sat up out of bed on Wednesday, fourteen days after the operation, and went to the country on Saturday the 16th October. She now lives in town, earning her livelihood as formerly, by binding shoes, but is often severely tortured with pain.

The reason why all of us were deceived in this women's case, was, the great obesity and distended fulness of the intestines, together with some protrusion pubic of the spine at the lumbar vertebræ. This did not at all appear conspicuous before operating, otherwise it should and must have struck some of the medical gentlemen who examined her; nor did it occur to myself during the operation, nor until some time after, when I could find no just cause for being so singularly deceived.

From this case, and those which I have enumerated, it appears to me that there is little danger to apprehend, in laying open the abdominal cavity; and that in diseased ovarium, extra-uterine conceptions, *fœtus in utero* with deformity of the pelvis preventing embryulcia, aneurism of the common iliac arteries or of the aorta, volvulus, internal hernia, cancer of the uterus, and foreign bodies in the stomach threatening death, we should have recourse early to gastrotomy. The delay in such cases is more dangerous than the operation. To show what freedom may be used in diseased ovarium, I have received, since writing the above, the following history of a case from my friend Mr Edward Scudamore, surgeon at Wye in Kent.

In 1821, A. C. 36 years of age, had been repeatedly the subject of *paracentesis abdominis* for ovarian or encysted dropsy, when fluid in increased quantities, and varying in quality in each operation, was drawn off. Her health declining, and her constitution resisting each effort to cure the disease, any proposition holding out the most remote hope was eagerly listened to. The trocar and canula were again introduced, the fluid drawn off, and the canula left with a plug inserted into its mouth. In a few days the plug was removed, and the accumulated fluid discharged, which operation was repeated for several successive times, after eight days interval between each. These attempts proving fruitless, and no irritation being produced by the canula, diluted port-wine was injected in one instance, and a solution of sulphate of zinc in the other; both of which merely produced a sensation of heat while they remained in the cavity. Many weeks elapsed after these operations, when the constitution gradually sinking, she expired.

III.

On the Muscularity of Arteries. By ROBERT HUNTER, Lecturer on Anatomy and Surgery, Glasgow.

THAT the arteries are endowed with a contractile power, and consequently can assist in the circulation of the blood, is a prevailing opinion, I believe, among physiologists. A few, however, of considerable reputation have questioned the truth of the opinion, and denied, *in toto*, the muscularity of arteries.

As the subject is of considerable physiological importance, I shall first examine the arguments that have been brought forward against the muscularity of arteries, and, *secondly*, mention a few facts and experiments which seem to lead to an opposite conclusion.

The arguments adduced against the muscularity of arteries may be arranged in the following order:

1st, That muscular fibres have never been detected in arteries.

2dly, That no kind of stimuli can make arteries evince a contractile power.

3dly, That what is commonly called the muscular tunic of arteries, is not a continuation of the ventricles of the heart.

4thly, That fibrine, the essential chemical constituent of muscle, does not exist in arteries.

5thly, That the arterial system has been ossified without deranging the circulation of the blood.

The first of these allegations, if well-founded, may seem to set the matter at rest. Are no muscular fibres discoverable, then, in arteries? If we appeal to authority, the answer to this interrogatory will be sufficiently unsatisfactory, for authorities are conflicting. While Bichat, Scarpa and others, have been unable to discover them, Munro, J. Hunter and Bell maintain that they have found them in man, as well as in many of the inferior animals. But relinquishing authority altogether, and admitting, for the sake of argument, that they have not been discovered in man, still this would be no proof of their non-existence. Muscular fibres differ so much in their appearance in different animals, and even in different parts of the same animal, that even though existing, they may not easily be recognised. The substance of the aorta probably differs less in appearance from common muscle than the texture of the snails or the oyster; yet this last is muscular, and acknowledged by all to possess that character, not indeed from the muscular fibres that can be easily seen, for some may be so blind as not to discover even here any thing like muscular fibre; but the motions with which these animals are endowed, are overpowering arguments, and as indisputably prove the existence of muscular fibres as the most palpable ocular demonstration.

This allegation, then, is not so formidable as might, at first view, be imagined; for even admitting that Bichat and others have not been able to detect them, still if we can prove that arteries are possessed of a contractile power, either the conclusion that muscular fibres exist, or the absurd alternative, that an effect may exist without a cause, would be unavoidable.

2dly, Bichat, however, maintains, that arteries possess no contractile power. Many phenomena, both in the diseased and healthy state of the body, tend to disprove this opinion. At all periods of life, the arteries can accommodate themselves to the quantity of the circulating fluids. When surcharged with blood, we find them dilated, and evincing, from an excess of stimulus, a strong full and frequent pulsation. When the quantity of blood, on the other hand, is diminished, we find the arteries still in contact with the sanguineous fluid; but, from the subduction of stimulus, exhibiting a small, soft, and weak pulsation.* This accommodating nature of the arteries to the quantity of the contained fluid, is rather a curious property of inert tubes.

In the dead subject, the arteries are admitted to be of greater diameter than when the blood is circulating through them. (Richerand.) This is just what might have been expected *a priori*, if the arteries were admitted to possess a contractile power, but is totally irreconcileable with their non-muscularity. The same phenomenon occurs in other tubes, respecting the muscularity of which no doubt exists. After death, the diameter of the intestinal canal is much increased; the tonic and contractile power of its muscular fibres are gone; and the tube hangs relaxed and dilated. The same phenomenon takes place in the arteries: is it, therefore, unreasonable to ascribe it to the same cause? While the blood circulates through them, it can only arise from the stimulus of the blood and the tonic action of their muscular fibre, that a certain degree of pressure is maintained upon the circulating fluid. When the vital spark becomes extinct, the contractile power is lost, and an increased diameter is the consequence.

If such a contractile power exist, it is but natural to suppose that we can increase it at pleasure; and this is absolutely in our power. All are acquainted with the numerous applications used by the ancients for arresting the progress of hemorrhage: Though we treat their practice now with derision and contempt, it would be absurd to deny the utility, to a certain extent, of these applications. I do not speak of cauterization and compression, but of those numerous cold and astringent lotions which were held in such repute, and the partial efficacy of which no man can doubt. If the bleeding arise from small arteries, these applications will instantly stop it; and the question then occurs, how is this effect produced? It must obviously arise from one of two causes:—either by inducing a coagulation of blood in the mouths of the vessels, or by exciting a contraction of their tunics. It cannot depend entirely upon the first of these causes, for the effect will follow, though the cold lotions be applied, not to the mouths of the vessels themselves, but at a considerable distance from them. Thus, the secondary hemorrhage that occurs after amputation, will frequently be stopped by applying cold water to the *skin* over the seat of the wound: and cold applications to the *scrotum* will relieve bleedings at the *nose.*

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* Hunter on the Blood, &c. p. 200.

The contractile power of the arteries may still farther be deduced, from a fact that is well known to every practical surgeon. An artery that can be discovered through the integuments pulsating with considerable force, when exposed to the cold air, loses in part, and sometimes altogether, its pulsation. A fact of a similar kind has been mentioned by Mr J. Hunter, respecting the arteries of inferior animals. After exposing the tibial artery of a dog, Mr Hunter found that the pulsations became weaker and weaker, till they ceased altogether. * These facts, I presume, can only be satisfactorily explained on the supposition that arteries are muscular, for it would be absurd to suppose that cold air contracts them as it would a piece of inanimate matter; no such contraction follows the application of cold to the dead arteries. If we admit, however, the existence of muscular fibre, the phenomenon can easily be explained. The cold air, as a stimulus to the muscular fibre, induces at first a slight contraction; but by the continued action of the stimulus, the contractions are urged on till the diameter of the artery is nearly obliterated.

I will freely admit, that many chemical and mechanical stimuli will produce no appreciable contraction of their tunics; but though we were more certain than we are that many stimuli would produce no effect whatever, this, I contend, would be no proof of their non-muscularity, for some muscles contract by a specific stimulus only. The iris, we have reason to believe, is muscular, for in many animals its movements are under the influence of the will, like the other muscles of voluntary motion: † still the iris can be made to evince no contractile power by any artificial stimuli that can be employed. ‡ But are we certain that the arteries are insensible to every kind of stimuli? The facts stated above, and still more the experiments of Dr Hales, militate against such an opinion.

Dr Hales fixed a pipe in the descending aorta of a living animal, and raised a column of water till it produced a power equal, as he supposed, to the force of the heart. He then cut a few small vessels on the intestine, as far from the mesentery as possible. A certain quantity of water flowed from the cut vessels in 46 seconds, while the same quantity of brandy, with an equal pressure, took double that time. By a similar set of experiments, he discovered that warm water passed more readily than cold water.

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* Treatise on the Blood, p. 200.

† Bell's Anatomy, vol. iii. p. 45.

‡ Bell's Anatomy, vol. iii. p. 48.

These experiments lead directly to the conclusion, that arteries are stimulated, in various degrees, according to the nature of the stimulus;—the brandy being more irritating than water, and cold water more stimulating than warm, a contraction had been induced, the calibre of the arteries diminished, and, of course, less fluid could pass through in a given time.

From these facts I think we are warranted to conclude, that arteries are possessed of a contractile power, and, consequently, of muscular fibres, by which these contractions are induced.

3dly, The fact, " that what is commonly called the muscular tunic of arteries is not a continuation of the ventricles of the heart, " proves nothing. As an argument against the muscularity of arteries, it is perfectly ridiculous. Let us place the syllogism in its proper form, and this will be apparent. The ventricle of the heart is muscular; the aorta is not a continuation of the ventricle, *ergo*, the aorta is not muscular. By a similar kind of reasoning we could prove that the ventricle itself is not muscular, for it is not a continuation of the auricle. In the fœtus, the auricle and ventricle are distinct bags, which soon become united by condensed cellular substance; but the muscular fibres are not continued into each other. * It is inconsistent with the economy of the circulation that it should be so. They are two separate muscles, not acting simultaneously, but in succession. If the reason is obvious why the ventricle should not be a continuation of the muscular fibres of the auricle, the reason is equally clear, why the ventricle and aorta should be likewise separated. When the auricle contracts, the ventricle must be in a state of relaxation to receive the blood; and when the ventricle contracts, must not the aorta be relaxed for a similar purpose? Were we warranted to draw any conclusion, then, from the fact that the aorta is not a continuation of the left ventricle, it would be one, I presume, rather in favour of the muscularity of arteries than against it.

4thly, Whether fibrine form an ingredient in the chemical composition of the arterial tissue, I will not take upon me to determine. Animal chemistry is still in its infancy; and although Berzelius and others who have extended the boundaries of this branch of knowledge, have not been able to discover it, still this is no decisive proof of its non-existence. Till very lately, fibrine was supposed to exist exclusively in animal substances. Vauquelin, however, has detected it in vegetables. * Fibrine, it is obvious, may exist in such tissues, but so obscured by other animal products, that the science of the profoundest chemist upon earth may be baffled to detect it. If, indeed, the chemical analysis of the arterial tissue had been proved to be absolutely correct, and no fibrine found, the argument would carry with it some little weight; but so long as the analysis is confessedly imperfect, it would be illogical in the extreme to draw from such premises any conclusion either for or against the muscularity of arteries.

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* Carson on the Motion of the Blood, p. 101.—Bell's Anatomy, vol. i. p. 460.

5thly, That the arteries have been partially ossified, without deranging, to any great extent, the circulation of the blood, is a fact which is well authenticated. We are not at liberty, however, to draw the conclusion from this fact, that the arteries in the healthy state are not muscular, or cannot assist in the circulation of the blood. We know how nicely the system can accommodate itself to the exigencies that arise both in health and disease. If an external sense is destroyed, the deficiency is partially compensated for by an increased acuteness of all the others. If an arm or a leg is lost, the organs of motion that remain generally increase in size and muscular power. So it is with the heart and arteries. When the heart is preternaturally small, changed in texture, or, as in some monsters, wanting altogether, the arteries always display an increase of strength by the thickness of their parietes, and their stronger contractile power. When the texture of the arteries, on the other hand, is so changed by disease as to render them incapable of assisting the circulation, the heart evidently acquires a stronger propulsive power than natural, as its increased magnitude in such cases demonstrates. Admitting, as we cordially do, that the arteries, to a certain extent, have been ossified without a great derangement of the circulation, the concession would be too broad and unguarded did we admit that the circulation of the blood would remain unaffected by the complete ossification of the whole arterial system. Ossification to such an extent, I believe, never has existed. The arterial *trunks* have indeed been found in this state, but the smaller branches, and the capillary extremities, could not become ossified without the immediate destruction of life. The work of nutrition, in such a state of the vessels, would be annihilated, and those numerous secretions which conduce to the necessities of the system, would be changed, or permanently arrested. Those who have supposed that the circulation is not affected by the ossification of the arterial system, have fallen into an egregious error;—the pulsation of the arteries is irregular and almost lost,—the whole system evinces powerful effects from so much derangement,—the patient is unhealthy, and of a remarkably sallow complexion, * and, from the imperfect distribution of the blood, mortification frequently supervenes. †

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* Ure's Chemical Dictionary, Art. Fibrine,

Secondly, The *action*, and, consequently, the muscularity of arteries, may be deduced from the following facts and observations.

1*st*, The pulse is not uniformly synchronous with the contraction of the left ventricle of the heart. Thousands of cases are on record to substantiate this fact. Such inaccordancies between the action of the heart and arteries, have been found to arise from various causes,—from deranged and organic affections of the heart,—from diseases of the arteries,—and even from diseases of organs which have only an obscure and inexplicable sympathy with the arterial system. Thus the phenomenon of a double pulse has been discovered to attend a morbid contraction of the left auriculo-ventricular opening of the heart; ‡ from ossification of the aortic valves, the heart has been known to contract twice for each arterial pulsation; § in whitloe, the arteries which lead to the disease sometimes pulsate more frequently than the arteries in any healthy part of the body; ‖ and the pulse has become extinct at the wrist and other parts of the body, from diseases of the alimentary canal and pulmonary organs. ¶ These facts are totally incompatible with the

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* Burns on Diseases of the Heart.

† Mr White of Manchester, when speaking of gangrene, makes the following observation. "There is a species of mortification from an ossified artery, which has ever been, and ever will remain, the *opprobrium medicorum.* The whole art of medicine is here in vain exhausted, and the complaint remains uninterrupted in its fatal progress, till it arrives at the extremity of the ossification."

‡ Hodgson on Diseases of the Arteries and Veins.

§ Burns on Diseases of the Heart.

‖ Good's Study of Medicine, vol. ii. p. 6.

¶ Parry on the Nature, Cause, and Varieties of the Arterial Pulse, p. 140.—The following facts from Dr Parry may prove interesting:

"Twenty cases have occurred to me of the total loss of pulse in the radial arteries, from maladies of the alimentary canal, whether a considerable time before death, or under circumstances that admitted of a recovery, while at the same time the pulse in the carotids was full and strong. In one instance of general dropsy, the patient had no pulse at the wrist for 17 days, yet was restored to perfect health. Sometimes the pulse has been wanting in the radial artery only. In another case, a young man labouring under pulmonary hectic was found to have lost the pulse at the wrist immediately after coming out of the warm bath. Several months after-supposition that the arteries are passive instruments in the circulation. They are, indeed, anomalies of pulsation, arising from some morbid condition of the system; but they are not the less valuable on that account, for it is only from attending to the condition of parts in disease, that we are often enabled to arrive at a correct knowledge of their healthy function. Were the arteries naturally passive instruments in the circulation, it is inconceivable how any disease could confer upon them a motive power; but if such a power primarily existed, even in the most obscure degree, it would require no great stretch of fancy to imagine how such a power could either be increased or annihilated. *

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wards, it had returned, although almost in an imperceptible degree. Another patient, a female of middle age, the mother of several children, affected with severe cough, was apparently convalescent and walking about the house, when it was discovered that the pulse in one arm was wholly wanting; a few days afterwards she died suddenly, when the whole course of the aorta was carefully examined, but no deviation from the healthy state could be perceived."

* Respecting the cause of the pulse in the healthy state of the body, much difference of opinion exists. Galen among the ancients, and Dumas among the moderns, supposed that the arteries had an inherent pulsation, independent of the action of the heart. Richerand, Portal, Sœmmering, and others, adopt the opinion of Haller, who maintained, that the phenomenon was attributable to the impulse of the blood from the left ventricle distending the arteries. "D'abord je me suis assuré, que le sang, poussé par le coeur, dilate les arteres et forme ce battement qu'on appelle le pouls." (*Memoir sur le Mouvement du Sang, p.* 33). Bichat conceives, that no dilatation of the arteries during the systolé of the heart takes place, but that the pulse is dependent upon a certain degree of locomotion in the whole artery, by which it springs against the finger during the contraction of the heart.

Dr Parry, by a variety of ingenious experiments, has attempted to prove, that the arteries suffer no dilatation nor contraction during the systolé and diastolé of the heart, nor locomotion of any description: that the pulse is dependent, therefore, solely upon the action of the heart.

"In a ram," says Dr Parry, "both carotids were laid bare by Mr George Norman, in the presence of Mr Coombs, two others, and myself. Notwithstanding the animal was greatly agitated, there was not in either carotids the least appearance of dilatation during the systolé of the ventricle, nor contraction during its diastolé: nor, except when the animal breathed, was there any degree of locomotion perceived in the arteries, which remained completely quiet and at rest. Nevertheless, when either artery was compressed between the finger and thumb, the pulse was very strong and distinct."

None of the above-mentioned explanations appear to be correct. The pulse, I conceive, is dependent on the action both of the heart and arteries.

When the heart contracts, a column of blood is sent into the aorta, which, from the incompressible nature of that fluid, must communicate an impulse at the same moment to all the blood in the arterial system, as far, at least, as the capillaries. This, to a certain extent, is the cause of the pulse; but unless the arteries react upon the blood impelled into them by the heart, the pulsation is obscure and indistinct, as we find where ossification attacks the vessels. In the experiments of Dr Parry, indeed, no perceptible dilatation or contraction could be discovered. But we ought to remember that the arteries, in all these experiments, were in an unnatural state;—they were necessarily exposed to the atmospheric air; and from the stimulus of that fluid, the muscular fibres, if such existed, would be called strongly, if not spasmodically, into action. It is to this circumstance, I presume, that an artery

2dly, The arteries after death are always found empty.

In the living subject, the arteries are full of blood. If the blood, therefore, be propelled by the contractile power of the heart alone, the moment the heart ceases to contract, the blood would remain motionless, and the arteries full. To what, then, can the vacuity of the arteries after death be attributed?

This state of the arteries after death, was one of the most powerful arguments which the antagonists of Harvey adduced against his transcendant discovery; and, anxious as Harvey was to explain the phenomenon, his explication is quite unsatisfactory. He supposed the vacuity dependent on a collapse of the lungs, which prevented the blood from passing through them to the left side of the heart, the left side still continuing to contract after it had ceased to receive blood. *

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whose pulsations were obvious to the sight, loses, when exposed, all apparent movement.

Dr Parry would almost lead us to conclude that the arteries cannot contract, as he maintains that they are always in a state of distension, as well under the systolé as the diastolé of the left ventricle. Some of his experiments would seem to lead directly to such a conclusion; but that arises from the imperfect state of the experiment. If the experiments had been followed out, as in a few instances they have been, they would lead to a conclusion in direct hostility to the one which has been deduced. For example, we are told that two ligatures were applied to the carotid of a living sheep, about an inch and a half distant from each other. Midway between the ligatures, the artery measured half an inch in circumference. After the blood was evacuated from the artery, it only measured $\frac{160}{400}$ of an inch; and its circumference, of course, reduced $\frac{40}{400}$ of an inch by the mere evacuation. The diminution of diameter from the evacuation of the blood, may seem a proof that the artery was previously in a state of unnatural distension; but the arteries, as I have formerly observed, have a capability of accommodating themselves to the quantity of contained fluid. If you diminish that fluid, the arteries will contract; and if the fluid be removed altogether from any one of them, the contractions will slowly and imperceptibly go on, till it is converted into an impervious cord. The decreased diameter of the arteries, under such circumstances, is no proof that they were naturally over-distended: it is only a proof that, in the unnatural circumstances in which they were placed, they were called violently, and, if we might so speak, unnaturally, into action. If the experiment had been followed out, by killing the animal, and sometime afterwards examining the arteries, the experimentalist would have found this contractile effort gone, and its diameter not only as great, but greater, than before. For the truth of this observation, I have only to appeal to one of Dr Parry's own experiments. In another experiment, he informs us that the circumference of the right carotid of a horse was $\frac{316}{400}$ of an inch. A portion of it was included between two ligatures, as in the above-mentioned experiment. An hour afterwards it was punctured; but, from the blood being coagulated, serum only flowed out. The vessel was then measured, and found to be only $\frac{252}{400}$ of an inch in circumference. The experiment, in this instance, did not stop here. The animal was bled to death; and in 16 hours afterwards, the artery, at the same point as before, measured $\frac{372}{400}$ of an inch.

* Vacuitas arteriarum in corporibus mortuis inde evenit, quod quando subsident (meatibus occlusis) pulmones, non ulterius respirant; ita per ipsos sanguis libere non potest transire: perseverat tamen per temporis spatium cor in expellendo; unde et sinistra cordis auricula contractior et ventriculus; pariterque arteriæ inanitæ et (non sanguinis successione repletæ) vacuæ apparent.—(*Exercitatio altera ad Riolanum, p.* 115. *Edit. of the London College*, 1766.

The recent experiments of Dr Williams prove, indeed, that the blood, after respiration has ceased, will not permeate the lungs, but it is not from any mechanical obstruction which they oppose to its progress, but from the impure state of the circulating fluid, *—impure blood acting upon the pulmonary veins as a torpifying poison. To suppose that the left ventricle, after it has ceased to receive blood, can propel the blood contained in the arteries into the veins, is the most unfounded of all opinions. It may, and indeed it does, contract for some time after the animal ceases to respire, but unless it at the same time propel a column of blood against a preceding column, how, in the nature of things, can its mere contraction affect any fluid that may be contained in the arteries?

Dr Carson has attempted to account for the phenomenon, from the natural resiliency of the lungs, which, after death, allows the blood to accumulate there as in a vacuum. †

Plausible, and even correct to a certain extent, as this view of the matter is, it is liable to objections. In birds, whose lungs are bound down to their back-bones, no resilience can take place. In fishes, too, no tendency to a vacuum can be formed, for they are destitute of lungs. How, then, can the vacuity in the arterial system of such animals be accounted for? From certain experiments which I some time ago performed, I was induced to believe that the vacuity was attributable to the contractile power of the arteries alone. A more extensive and correct set of experiments obliged me, however, to abandon that opinion. These experiments, notwithstanding, prove, what is sufficient for our present purpose, that the arteries are concerned, and assist in the process.

To ascertain the cause of the vacuity in the arteries, the following experiments were instituted.

Experiment 1.—The thorax of a living cat was opened on the 22d of January last, in presence of a number of my pupils, by cutting through the cartilages of the ribs on each side of the sternum. The lungs instantly collapsed, with a considerable noise. The heart was then seen beating irregularly, but pretty strongly, through the thin pericardium. The pericardium was slit open, and a ligature applied around the roots of the systemic and pulmonic vessels, just where they issue from the heart.

Upon examining the animal 12 hours afterwards, the veins were found remarkably turgid, and the blood in many of the larger ones coagulated. The aorta was next examined throughout its whole extent, and some of its branches likewise, to their minute ramifications. It was flatter and more flaccid than when the animal was alive, or than it appeared for half an hour after the death of the animal. Having opened the aorta, it was found about half filled with thin uncoagulated blood; and as we traced the branches which proceed from the aorta, we found them still partially filled with the same kind of blood.

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* Edinb. Med. and Surg. Jour. Vol. xix. p. 526.

† Medico. Chirurg. Trans. Vol. ii. Part 1st.

Exp. 2.—Next day the thorax of another living cat was opened, and a ligature applied round the aorta and pulmonary artery; the *venæ cavæ* were not surrounded with a ligature. Twelve hours afterwards, the vascular system was minutely examined. The right auricle and ventricle were preternaturally loaded with blood, and the veins turgid. From the heart nearly to the cœliac artery, the aorta was empty; but beyond that point the arteries contained a small quantity of uncoagulated blood.

Exp. 3.—On the 25th of January, the thorax of a living cat was opened, when the lungs collapsed, and the heart continued to pulsate irregularly for some time, as in the foregoing experiment. A ligature was then applied around the root of the aorta, no other vessels being included. Six hours afterwards, the arterial system was found nearly in the same state as in the second experiment.

Exp. 4.—On the 1st February the thorax of a living cat was opened, and a ligature applied round the aorta, as in the third Experiment. An incision was next made into the trachea, and the lungs forcibly inflated and the trachea tied. After the animal had become cold, the arteries were examined and found perfectly empty.

The object of the first experiment was to insulate the vascular system from the heart and lungs, and to observe the effect. Had all the blood been found in the venous system after this experiment, the vacuity of the arteries would have been attributable to their own contractile power; but as we found a considerable quantity of blood in these tubes, we are forced to the conclusion, that the arteries of themselves are inadequate to the effect. That they assist the process, however, is indisputable from the result of the experiment; and the question then occurs, what other power or powers cooperate with the arteries in producing the effect? In the second experiment, another power, viz. the right side of the heart was added, and the result was considerable; still the arteries were only partially emptied. In the third experiment the lungs were added, but no farther effect was produced. Upon considering the matter, I remembered that the lungs from the entrance of the atmospheric air into the chest, had been, during the whole experiment, in a state of collapse. In the fourth experiment, this objection was obviated by inflating the lungs, and the result was such as has been mentioned.

The vacuity of the arteries after death, then, is attributable to three causes; 1*st*, To the contraction of the arteries; 2*dly*, To the suction-power of the right side of the heart; and 3*dly*, To the natural resilience of the lungs, producing a tendency to a vacuum. These three causes appear indispensable to produce the effect. If any one fail, the arteries remain comparatively full. When the arteries are ossified, so that they cannot contract, or when the irritability of the heart is suddenly destroyed by the electric fluid, or by any other agent, the arteries remain partially filled with blood.

3*dly*, The contractile power of the heart has been lost by organic disease, without affecting to any great extent the circulation of the blood.

Mr John Hunter, has justly remarked, that the heart cannot be an essential organ of the circulation, since it is wanting in so many animals. By a parity of reasoning, I think we may conclude, that even in those animals in which a heart naturally exists, it cannot be the sole circulating agent, as its contractile power has been destroyed without a cessation of the circulation. That the heart should be liable to many diseases, both functional and organic, is by no means surprising. It stands associated to so many organs by situation and sympathy,—is so easily affected by every mental and corporeal excitation, that its action is incessantly liable to derangement; and functional diseases are known to terminate frequently in textural or organic. Many curious cases of this kind are on record; and as they form the basis upon which our present argument is founded, I shall briefly glance at one or two of them.

In Mr Allan Burns's work on Diseases of the Heart, we are informed that one of his patients was for many years afflicted with a disease of the heart, which frequently became so urgent, that he dropped down apparently dead; his pulse ceasing to beat, and his face becoming livid and swollen. On the afternoon preceding his death, he had for a little time been engaged in some laborious employment, which completely exhausted him. He went to bed earlier than usual, and was found dead about 11 o'clock the same evening. On inspecting the body, the ventricles of the heart were found completely changed in structure, being now composed of a substance "intermediate between fat and cartilage."

The heart has been found disorganized in a still more wonderful degree. It has been found converted into solid bone. Several cases of this kind will be found by referring to Burnet, Baillie, Good, Bordenave, Simmons, and Burns. An interesting case of this kind is related by the last mentioned eminent author, of a woman, whose disease at first seemed to consist principally in a cough and difficulty of breathing; her pulse being regular and appetite unimpaired. For two months previous to her death, she was afflicted with nausea, great constriction in the chest, and diffused pain over the abdomen. She died; and on examining the body "the whole extent of the pericardium which covered the ventricles, and the ventricles themselves, except about a cubic inch at the apex of the heart, were ossified and firm as the skull."

In the case related by Dr Simmons, the ossification extended from the base to the apex of the heart, and consequently involved both auricles and ventricles.

Here, then, is matter for serious consideration. In such cases, the heart must have been useless for years; yet the circulation was not only maintained, but maintained with such regularity, that the disease was frequently overlooked or mistaken. Either these cases are unworthy of credit, or the heart cannot be the sole agent in the circulation. Were such cases adduced by men of no reputation in their profession; were they adduced by men eager in the support of some favourite hypothesis, we might be inclined to receive them with caution. But, brought forward as they are by men of the first respectability; warped by no theory, and influenced by no hypothesis; who state, in the calm language of reason, facts which have fallen within the sphere of their sober observation—such facts are entitled to our implicit confidence. We are forced then to adopt the only alternative, that the circulation must be carried on under such circumstances by other agents than the heart; and where can these agents be found, but in some of the remaining apparatus of circulation? When the arteries are ossified, we readily admit that the blood is moved through the arterial system by the percussive action of the heart; and when the heart is ossified, and the arteries sound, have we not, at least, a strong presumptive proof that the sanguineous movement is dependent, under such circumstances, upon the contractile power of the arteries?

4thly, The heart is wanting in many classes of animated beings in which a circulation exists.

We are apt to consider these animated beings as of a less perfect organization than ourselves, but with how much reason, is difficult to determine. They may be less complex in structure, but simplicity of organization is surely no proof of inferiority. Upon the same principle, the organ of smell may be denominated less perfect than the eye; but, though the one is less complex, or, if you please, less noble and useful than the other, yet, as both are equally fitted for their several functions, both are equally perfect. So it is with "the lower animals." The relationships which they bear to the external world are different from ours. We are placed, by a peculiar organization, on a mighty vantage ground; but, so long as these animals can perform the parts assigned them in the general arrangement of the universe, they are as perfect as we. Be this as it may, by investigating the structure of these animals, we obtain a knowledge of many important facts in physiology. In framing many of the human organs, nature has displayed all her exuberance of power. In other animals, however, we find her unravelling, as it were, her own intricacies;—removing first one part, and then another, till the essentials only of the organ remain. By thus tracing Nature through her "sportive movements," we receive much both of pleasing and profitable instruction. While we are charmed with varied and exhaustless resources, we are no less edified by the correct views of the animal economy, which such exercises are so calculated to impart. How are we, for example, certain that the auditory nerve is the only essential part of the organ of hearing, and that all its other tissues, exquisitely delicate and useful though they be, are comparatively unimportant appendages? Chiefly because comparative anatomy has proved it.

If we appeal, then, to the same tribunal in deciding the essential parts of the apparatus of circulation, we will assuredly be led to form a very different opinion from that entertained by the generality of physiologists. Do we say it is the heart? Comparative Anatomy instantly confutes the assertion, by pointing out a numerous class of animals in which a circulation exists, without any such organ. Those beings which possess a heart and circulation like man, are indeed but a handful compared to the countless millions that inhabit the ocean; the bowels of the earth; or people the atmosphere, and flutter in every sunbeam. The great majority of such animals are destitute of a heart; yet can we believe that their circulatory apparatus is not adequate to every useful purpose in their economy? Does not the blood move, and are not the functions of nutrition, conservation and reproduction, executed with as much regularity as when the heart is attached to the sanguiferous system? Did we find a heart in all animals in which a circulation existed, the conclusion that it is an essential organ of the circulation would be unavoidable; but, varying in structure, as it does in so many animals, and wanting in part, or altogether, as it is in so many more, we are forced to draw a very different, if not an opposite conclusion. That the heart is a most useful organ in those animals in which it exists, is undeniable:—it is a useful reservoir for the blood-vessels; and, by its strength and excessive irritability, is admirably adapted for maintaining in the vessels a constant and equable supply of blood, as well as powerfully assisting the sanguineous movement. That it however is the *only* agent of the circulation, is a gratuitous assertion. There is no *corporeal* heart for the fish, yet the blood circulates with ease through its *body;*—no pulmonic heart for the snail, yet the blood stagnates not in its respiratory organ;—no heart, either corporeal or pulmonic, for worms and certain insects, yet the blood experiences no interruption in its course;* and, even in man and quadrupeds, in whom the heart might seem at first view the only organ of the circulation, we find the blood moving with uniformity in the system of the vena porta, where the action of the heart cannot possibly extend. In some of her playful moments, Nature too has enabled us to prove, that the blood can circulate over the whole human system, independently of that organ; for monsters have been born without a heart, and have lived for some time, the blood circulating with apparent regularity.

In all these instances, then, unless the vessels be admitted to be the principal moving powers in the circulation, how can the sanguineous movement be at all accounted for?

5thly, Different parts of the arterial system may be at the same moment in different states of excitement and action.

We cannot contemplate the sudden and deep blush of shame, or the pallid hue of terror, without admitting the truth of this observation.† Do we not likewise find the blood circulating with more impetuosity in an inflamed than in a healthy part; and rushing with great force into the penis during the erectile state of that organ, while the circulation remains unaffected in other parts of the body? These are not the only facts which prove the truth of the above mentioned position.

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* Blumenbach's Comparative Anatomy, p. 247. B. F. Bening de Hirudinibus. Cuvier's Leçons, &c. I. 23. Sect. 2. Art. 4. Mitchel in Aliber's American Annals, Part I. p. 121.

† Abernethy's Physiological Lectures, p. 227.

We cannot observe the growth of any tumour, or indeed of any portion of the body, either in health or disease, or the states of action and repose of secretory organs, without being forcibly impelled to the same result.

The secretions are all formed from the blood; yet we find the secreting organs not only differing from each other in their secreting power, but each exhibiting likewise alternate periods of repose and high activity. The saliva does not always flow into the mouth, yet the idea of savoury food will instantly call it forth in copious profusion. The gastric juice is not always secreted; let food, however, be introduced into the stomach, and the digestive menstruum will soon appear. The testicle, too, has long and indeterminate periods of inaction; yet how easily can it be roused from this state into vigorous excitement!

That the vessels which lead to an active organ are unusually loaded with blood, is a fact which has been long known and universally admitted. We have even ocular demonstration of the fact in the living subject; and experiment has proved the increased magnitude of the vessels in the dead. The question then arises, what is the cause of this greater influx of blood? Is it attributable to the heart? Can the heart instantly direct a stream of blood to any organ that may require it, without transmitting, at the same time, a corresponding increase to all the others? The thing is impossible.

These, and many other phenomena of the animal economy, are quite inexplicable, unless we admit that particular arteries have the power of dilating and contracting, of directing and impelling the blood to particular organs; and if particular arteries have this power, with what reason can we refuse the same power to the whole arterial system.

24*th April*, 1824,
424, *Duke Street*, *Glasgow*.

IV.

An Inquiry on several doubtful Points connected with the Circulation. By James Macfadyen, Surgeon, Lecturer on Natural History, &c. Glasgow.

This is one of those subjects which, after having been for a long time laid aside, has of late come again to occupy a considerable portion of the attention of Physiologists. Notwithstanding all that has been said much still remains in uncertainty, and on which our received opinions are merely conjectural. We cannot, it is true, expect another discovery like that of Harvey, instituting, as it were, a revolution in the science, leaving the long-favoured notions of antiquity to be forgotten amid the new views and more correct opinions it shall introduce. Perhaps, in all the wide range of natural science, we have no theory the truth of which can be more certainly demonstrated, than the at present received doctrine of the circulation of the blood. Still, however, although the foundation on which we proceed may be correct, there are several opinions connected with it, regarded by many as incontrovertible, which I am confident, if properly examined, would be found incorrect, and many points on which we are still undecided, to explain and illustrate which, is an object well worthy of the attention of the Anatomist and Physiologist.

The remarks I will at present hazard, may be arranged under the two distinct heads of the Arterial and the Venous Circulations.

I. *Arterial Circulation.*

One of the most important questions connected with the Arterial Circulation, is the muscularity of the vessels themselves. I do not envy the discriminative talents of those persons who make themselves proud on discovering what they are pleased to call a new arterial coat. It is perfectly sufficient if we know that there are three distinct layers, a cellular, a muscular or fibrous, and a mucous. It is with the second of these that we have at present to do, our object being to ascertain whether it is truly muscular, or composed merely of elastic tendonous fibres.

One important objection to the former of these opinions is, that the arteries possess none of the properties which are considered characteristic of muscular parts. It is true, that in fish, and others of the cold-blooded tribes, the muscles may be white. Yet that does not apply to the case before us, those of warm-blooded animals being always red. If, therefore, the second arterial coat be muscular, how are we to account for its being so unlike, in appearance and texture, the other parts of the same system. Besides, contraction on the application of stimuli is supposed to be an essential property of muscles. This, however, on making the experiment, I am not aware has ever been observed. No sensible contraction takes place, and no increase in the velocity or force of the circulation in the part is discernible, One of the arguments considered the strongest in favour of the muscularity of these vessels is their pulsation. This, however, recent observations have ascertained to be altogether independent of their action. On laying an artery bare, no alternate dilatations and contractions are observable to the eye, and it is only when we apply pressure, so as to narrow the diameter of the canal, that we are sensible of the successive tides in which the current is moving. Hence the distinction with which the pulse is felt, is always in proportion to the firmness with which the artery is compressed, being always felt strongest where the vessel passes over some bone, as is the case with the radical artery at the wrist, and the temporal over the forehead. It hence appears, that pulsation is altogether independent of any muscularity in the arteries, and that they have a very subordinate share in the propulsion of the blood. This, indeed, may be put to the test of experiment. If one of the larger arteries be divided, and introduced into the aorta of a dead animal, or indeed into any inert tube, the blood will be observed, on compression, to flow through the latter with a distinct pulsation, and to escape by the orifice at the extremity *per saltem.* We may, indeed, convince ourselves of this by a very simple experiment—merely compressing the leathern pipe of the common fire-engine, when, if I may be allowed the expression, a very distinct pulsation will be felt corresponding to each stroke of the engine.

Another argument brought forward in favour of the opinion we are considering, is, that the arteries are always found empty after death. The inference has accordingly been made, that they are possessed of a propelling power independent of the heart. This conclusion is unwarranted; for, whilst the arteries, whose muscularity is so questionable, are empty, the right ventricle, though formed of one of the most powerful muscles in the body, as well as the pulmonary arteries, is gorged with blood. The only probable theory therefore is, that the blood, propelled through the arteries by the action of the left ventricle, partly passes into the veins, and partly remains in the vessels, and is there either gradually carried forward in the same manner by the fibrous contraction or elasticity of the vessels themselves, or, when putrefaction commences, escapes and forms those livid marks which are so often observed on the surface of the dead subject.

But how, it may be asked, comes the right side of the heart, together with the pulmonary arteries, to be gorged with blood, whilst the pulmonary veins and the left side, as well as the arterial system, are empty? This is not occasioned by the former possessing less muscular energy than the latter, or from vitality in them being sooner extinct. The true cause is, that the function of respiration ceasing, the blood being no longer purified by exposure to the vital air, retains those deleterious properties characteristic of venous blood, and is no longer capable of supporting life. It may therefore, in so far as the arteries are concerned, be considered as existing in an unnatural state,—as a foreign fluid differing essentially from that with which that system of vessels were formerly filled. The consequence is, that the pulmonary veins refuse, if I may use the expression, to take up the blood, being no longer in a state suited for the purposes of life. This may be illustrated by the spasmodic contraction of the *rima glottidis*, in the application of any unusual substance, such as either water, or any of the noxious gases. In these cases death takes place, not from the direct effects of the foreign substances themselves, but from the orifice of the windpipe being completely closed, as if a rope were tightened round the neck. So also, in the case before us, the pulmonary veins, from the unusual properties which the blood, after the cessation of existence, possesses, become incapable of receiving and transmitting it to the left auricle; so that whilst they, together with the rest of the systemic vessels, are empty, there is an accumulation of blood in the pulmonic, the lungs, from their lax texture, admitting readily of their distension.

As a farther confirmation of this, it may be noticed, that the arteries, in cases of sudden deaths, always contain more or less blood. Here life has been, as it were, arrested in the midst of her operations. She has not been even allowed space sufficient for her usual orderly departure, that gradual cessation of one function in the extinction of another. But the action of all have ceased at one moment; and there is but one step between being and nonentity, between existence and death.

But it may be asked, In what respect, if the statement we have just made be correct, do the arteries differ from the veins? In answer to this, we may, in the first place, reply, that their functions are not alike. While the latter are almost passive, the former, by their elastic property, assist in carrying forward the vivifying current. They also differ in structure, the coats of the veins being lax and delicate, whilst those of the arteries are firm and thick, readily assuming, when left to themselves, the tubular form. The latter, indeed, may almost be considered as the tendons of the muscle which forms the left ventricle, sent out to be inserted in every portion of the body.

It has, however, been said, that although the larger trunks are destitute of muscular contraction, it is possessed by the smaller branches, since they are provided with stronger coats. This also is erroneous. The coats of the smaller branches are even thinner than the larger; and when we say that they are stronger in the former, we must be understood as speaking comparatively as to the diameter of their canals. This strengthening of the extreme branches is intended merely to enable them to resist the impetus of the blood; since, if their walls were to diminish in thickness with the size of their cavities, they would, towards their extremities, be so thin, that rupture would necessarily be a frequent accident. The same thing may be also observed at the bifurcation of arteries, or when a branch is sent off, there always being an interlacing of fibres at the point where they divide, that being most exposed to the current as it flows from the heart.

II. *Venous Circulation.*

Having thus attempted to prove the non-muscularity of arteries, we may proceed to the venous circulation. The only question connected with it that we will at present notice, is that of the power which carries back the blood to the heart. In reply to this, various theories have been proposed.

The first we will mention is that which ascribes the circulation in the veins to the suction-power of the right auricle. According to this supposition, the heart not only propels the blood towards the extremities, but also recals it; and whilst by its contraction its cavity is emptied, it is only to be again filled by means of its expansion. In short, as Mr Carson expresses it, it is at the same time a forcing and a suction-pump.

This theory, though long ago suggested, is in a manner recent, being only of late seriously advanced and defended. In opposition to it, we have several objections to state.

In the first place, it is opposed by direct experiment. One of the large veins in the abdomen of one of the inferior animals was detached from its connexions, and a ligature applied. If in this state it had been replaced, and the wound brought together, there can be no doubt but that it would have become empty. By preventing however any pressure, it remained unaltered, continuing seemingly as full as when the ligature was applied. In another case, the *vena cava abdominalis*, shortly below where it passes under the liver, was exposed, and an opening made into it by removing a small portion of its coats, a ligature having previously been applied below the place where the incision was made. A determinate quantity of liquid was then introduced. At the end of ten minutes it was returned, but little diminished, though every care was taken that the passage to the opening in the vein should be free and unobstructed. *

And here a very natural question connected with the distribution of the veins of the membranous viscera which unite to form the *vena portæ*. Even did the suction-power with which the right auricle has been endowed by our modern physiologists extend to the rest of the system, this set of vessels is evidently beyond its reach. Receiving the blood from the mesenteric arteries, uniting into one trunk, which, entering the liver, is there again divided, and spread into minute ramifications, the motion of the fluid which they contain must evidently be beyond the influence of the action of the heart. This fact alone, one would think, should have been sufficient to have made our theorists pause ere they came forward with an opinion in every point of view so objectionable.

Another strong objection to the theory in question, is the weakness of the right auricle. Were it endowed with the power of sucking back from every part of the body, the blood which the left ventricle had discharged, it ought evidently to be at least of equal strength. On the contrary, whilst the latter is amongst the strongest muscles of the body, the former may be reckoned among the least powerful. We are even inclined to doubt whether its action is absolutely necessary to the expulsion of the blood into the ventricle, since, were it possessed of any strengh, the *venæ cavæ* being destitute of valves, a portion at least of the blood would, on each contraction, be driven back into the veins. The blood, therefore, passes into the ventricle, urged not so much by the contraction of the heart, as by the current of blood which is pressing forward to the same point from every part of the venous system.

But we have still another argument, founded on these mechanical principles, which seem to have suggested the theory under consideration. In the first place, it must be evident, that were we to attempt to draw up any liquid, by means of the common pump, through flexible tubes, their sides would collapse as soon as the weight or resistance of the liquid would exceed their elasticity. Of this any one may convince himself, by merely attempting to draw up water from a vessel through a tube of moistened paper, the sides collapsing long before even the air is exhausted. The same thing would take place, were the heart possessed of a suction-power, the sides of the veins uniting and becoming impervious as soon as its action began.

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* In a similar experiment of a medical friend, repeated by him several times, one end of a tube was introduced into one of the large abdominal veins of a dog, and the other placed in a vessel containing water. No effect, however, was produced; none of that suction-power was displayed of which we hear so much. This, however, cannot perhaps be considered as conclusive.

Besides this, it is another maxim in hydraulics, that the resistance is always in proportion to the diameter. If it would be difficult to raise water by means of a pump, through a pipe of a four-inch bore, it must be almost impossible to accomplish the same if the diameter were only an inch, and perfectly so if you diminish it still father. Now, as it has been already proved, that it would be impracticable for the heart to draw the blood through the *venæ cavæ*, what shall we say, when we turn to the more minute branches—the origins of the venous trunks? The principle we have laid down applies still more strongly here, since, even did the auricle succeed with the larger vessels, it could have no influence over the more minute ramifications.

In concluding this subject, we may notice, that it is one of the arguments insisted on by the favourers of the opinion we have been combating, that when a bandage is applied round a limb, while the veins below the part become turgid, those above are gradually emptied. How this can at all be referable to the suction-power of the heart, it is difficult to conceive. We see the veins below the part become turgid. This can only take place from the *vis a tergo* of the older writers, the circulation of the arteries going on, and continuing to pour blood into the veins that arise from them. As for the emptying of the veins above the ligature, this is affected, not by any influence exercised by the right auricle, but by an agent, which, I think, has never as yet been taken into account.

We do not deny that the circulation, at the extreme venous branches, may in a great measure depend on the constant influx of blood from the arteries. That this alone, however, is sufficient to carry the blood forward to the heart, in its course encountering so many obstacles, is what we can scarcely suppose. It must therefore require additional aid; and this it derives, neither from the heart, nor from any muscular or elastic property possessed by the veins themselves, but are indebted to that pressure to which these vessels are exposed in their course,—passing through or covered with muscles, and kept down firm by the numerous tendinous expansions, and the skin which invests every part of the body. At first glance, this theory may appear improbable and hypothetical. A very slight reflection, however, will show that it is far from being incorrect.

To be convinced of this, only observe the forearm. We have in the first place the skin, which, in a state of health, is firmly braced, forming an envelope to the internal parts. Next we have the fascia of the forearm, in like manner tightly binding down the fleshy portion of the arm. Lastly, there are the muscles themselves, by their contraction and relaxation, exercising a considerable influence on the fluid contents of the vessels which lie in contact or pass through them. The influence of this last, indeed, seems to have been provided against by nature; these not only being deep-seated, but also superficial branches anastomosing freely with one another. This is also confirmed by the presence of valves in the course of the veins. Were the *vis a tergo* the only power that gave motion to the blood in these vessels, then no valvular apparatus would have been required. On the contrary, the different causes we have just noticed, operating to diminish their diameter, the blood, were it not prevented, would have as great chance of flowing back, and being extravasated, as of being driven on towards the heart.

The same theory applies to the large veins of the abdomen and thorax. Here the weight of the different viscera, and the pressure occasioned by the constant tendency to contraction of the parietes, prevent the blood being retarded, and assist its progress, where, it would be equally beyond the influence of the suction-power of the right auricle, and the *vis a tergo*.

Such is the train of argument that may be employed to prove, that, so far as the arterial circulation is concerned, we must regard the left ventricle of the heart as the principal agent, that the arteries are destitute of muscularity, and it is by their fibrous elastic texture that they favour the progress of the blood. As for the venous circulation, we think it must be evident, that the suction-power of the right auricle is a fiction equally absurd as the notions which some entertain about the elasticity of the heart, and the contractility of the lungs. It is the *vis-a-tergo*, but more especially the pressure of the parts through which the veins pass, that are the real agents, in forwarding the blood to the great centre of the circulation.

Though some of the arguments which have been employed in this inquiry, may have claims to novelty, many of them may be found in the writings of the more eminent physiologists of our own time. The effect they have produced on my own mind, is the most perfect conviction of the truth of the opinions I have here advanced.

No. 32, *Ingram Street, Glasgow,*
June 13*th*, 1821.

V.

Observations on Mental Derangement, made in the Hospital Salpetriere at Paris. *By* H. BARDSLEY, M. D., one of the Physicians to the Manchester Infirmary, &c. &c. &c. late President of the Royal Medical Society of Edinburgh.

IN the hospital Salpetrierė at Paris, which contains between seven and eight hundred female lunatics, the best possible opportunities are afforded for witnessing every variety of mental hallucination, from hypochondriasm and melancholy to the most furious mania. The aberrations of the human intellect are there strikingly exhibited. On entering an institution of this kind, it is impossible to suppress those feelings of sympathy and commiseration, which are produced by the sight of so many individuals deprived of that faculty which is the source of man's most elevated enjoyments. I confess I was highly gratified with the regularity and neatness which prevailed in every department of this spacious infirmary for the mind. The distribution of the different classes of patients into separate divisions of the building is attended with many advantages. The taciturn, playful and harmless, are, by this regulation, relieved from the noise and danger to which their intercourse with the more furious and irascible patients would necessarily expose them. The moral treatment is conducted on the principle of mildness and persuasion. This change is chiefly to be attributed to the exertions of the venerable Pinel, whose example is followed by his able and very intelligent successor, M. Esquirol. The severities formerly exercised towards these pitiable creatures, were not only discreditable to the humanity of their medical attendants, but injurious to the patients themselves in a curative point of view. On this subject, M. Pinel thus feelingly remarks: "Les aliené̄s loin d'être des coupables qu'il faut punir, sont des malades dont l'état pénible merite tous les egards dus à l'humanite souffrante, et dont on doit rechercher par les moyens les plus simples à retablir la raison egarée."

One leading aim in the moral management is to prevent ennui, and to divert the mind from brooding on the visionary fantasies of its own disordered imagination, by affording the patients regular and suitable employment. The medical treatment appeared to me inefficient and defective. So much stress is laid on the paramount importance of moral management, that active remedial means are almost precluded. Nature is left, in a great measure, unassisted by the powers of medicine. I am at a loss to account for this inert practice, as it is opposed to the opinions delivered in the writings of the very physicians who adopt it. They admit the influence of morbid bodily actions in producing insanity, and yet disregard those agents which are most likely to remove them. In cases of organic alteration of the brain, it must be confessed that we cannot reasonably expect to derive much benefit from medicine; but daily experience confirms the utility of a judicious employment of remedies in many cases of mental alienation, in which moral management has been inefficaciously resorted to for relief. Few practitioners in this country, who, from their connexion with receptacles for the insane, have enjoyed opportunities of witnessing the influence of medical treatment in insanity, will, I imagine, agree with M. Georget in the following assertion: * " Qu'a l'exception d'un petit nombre de cas, où l'on ne peut contester leur effets salutaires, (des medicamens), ils ne peuvent nous servir que faiblement à changer, ou à modifier bien sensiblement la succession des phenomenes, " &c.

That attention is not paid to the functions of the digestive organs, which their importance in the mental balance seems to indicate.

Dr Spurzheim very justly remarks, † " that the greatest number of cases of insanity, produced by sympathetic causes, originate from deranged functions of the digestive organs,—that sort of insanity which is very seldom cured by nature alone, and is not sufficiently understood by medical practitioners." Tamarind water, acidulated whey, and various emulsions, supply the place of more active and useful remedies. Purgative medicines are prescribed in a manner not at all likely to ensure the acknowledged good effects which attend their regular and judicious exhibition in many cases of mental affection.

Pinel, Esquirol and Georget, speak unfavourably of the efficacy of venesection; and, indeed, this operation is rarely employed in the Salpetriere. Emetics are not in very frequent use. The cold bath is almost exploded from the practice of the hospital. The tepid bath, on the contrary, forms a very essential part of the medical treatment. Such patients as are selected by the medical attendant, enter the bath twice or thrice a week, or as frequently as he may judge proper. The

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* Georget, De la folie, p. 300.

† Observations on Insanity, p. 300.

period of immersion varies according to the effects wished to be produced; but it seldom exceeds one hour, and is rarely less than half an hour. I have witnessed its beneficial effects in many instances. The system of pumping, or of directing a stream of cold water upon the head, from a height of three or four feet, whilst the body is immersed in tepid water, is not so much practised by M. Esquirol as it was by his predecessor M. Pinel; for he is decidedly of opinion (and this opinion is derived from long experience) that it seldom produces any permanent good effects. His pupil and assistant, M. Georget, mentions his having known "its continued use to produce disorganization of the *cerebrum*, and to determine the incurability of insanity in a number of cases." When every other attempt to enforce tranquillity and mild behaviour on the part of some of the more mischievous and unmanageable patients, has failed, pumping is occasionally employed as a last resource, and sometimes with the desired success. This mode of punishment is somewhat harsh; but, if applied with due care and moderation, it will be found not altogether deserving of trial. In the following case, as in several others, its use was attended with a favourable result.

A young Brunette, of robust habit of body, had been for several years subject to violent exacerbations of insanity for a week or ten days preceding the menstrual discharge. Threats, bodily punishment, and various modes of coercion, had been employed to deter her from unremitting attempts to commit suicide; but on several occasions she had nearly succeeded in her dreadful purpose. At the desire of her friends, she was admitted into the Salpetriere. As soon as this state of fury supervened, she was ordered to be conducted to the bath, and to have cold water applied to the head in the manner above alluded to. Its effects were striking; for though a moment before she was vehemently vociferating execrations against her friends and all present, and using violent means to extricate herself from her novel situation, when the shock came she seemed in a moment to have lost both mental and physical force. In about ten minutes, the paroxysm returned, and the affusion was again repeated with a similar result. It was found unnecessary the succeeding month to resort to this practice; for during the height of her excitement, the mere mention of the bath was sufficient to check it, and render her tranquil and composed.

The food, clothing and bedding, are excellent in their kind. The use of the wooden clogs appears to me objectionable; for though they preserve the feet dry, still, in a paroxysm of maniacal fury, they may become the instruments of severe and fatal mischief.

It would be superfluous to enter into a more particular account of this Institution, as Pinel, Esquirol and Georget,* have given such minute and accurate descriptions of it in their publications. I shall therefore merely transcribe from my notebook such cases, with the *post-mortem* appearances, as seem most likely to throw a little light on the pathology of mania.

Barbara —— ætat. 43, had laboured under periodical fits of insanity for five years. Her conduct and conversation evinced at times great mental composure. Her feelings towards her parents and her friends, which, in her lucid intervals, were tender and affectionate, seemed, during the maniacal paroxysm, to undergo a complete change, for she constantly spoke of them in terms of hatred and contempt, and represented their conduct as unjust and barbarous. This state of perturbation generally continued for a few weeks, and was afterwards succeeded by a behaviour calm and decorous. She had been occasionally subject to epileptic fits for several years, which recurred much more frequently for some months previously to her death. She was seized with cough, dyspnœa, and œdema of the inferior extremities, which symptoms during the last four months of her life, were accompanied with copious purulent expectoration, and occasional hæmoptysis. Her strength rapidly failed, and she died a victim to confirmed phthisis. *Dissection.*—The cranium was extremely thin. The dura mater was quite natural. There was no congestion in the vessels of the pia matter, and the choroid plexuses were colourless. The lateral ventricles contained a small quantity of serum. The lateral lobe of the left hemisphere of the brain had assumed a fibro-cartilaginous texture; and it was found, upon minute examination, that the temporal fossa of that side was so much less than its opposite, as necessarily to have compressed the portion of brain it contained. The anterior and posterior lobes, the right hemisphere, the cerebellum, and the medulla oblongata were perfectly natural. On opening the thorax, adhesions were found between the pleuræ costalis and pulmonalis, but they were readily separated without laceration. The substance of the lungs externally appeared healthy; but, on dividing the right lung with the scalpel, two vomicæ of considerable size, each lined with a semi-cartilaginous membrane, and irregularly intercepted by fibrous bands, were discovered in its upper and middle lobes. The upper lobule of the left lung was much consolidated, but the inferior lobe was in a natural state. The *bronchi* of the right lung were filled with purulent fluid, those of the left presented no unusual appearance. The other cavities were not examined.

A woman, ætat. 45, had been an idiot during 15 years, and continued in this state of fatuity until her death. She had constantly complained of obtuse pain in the frontal region, and required the daily exhibition of drastic purgatives to obviate excessive costiveness. *Dissection.*—The pericranium adhered firmly to the skull, but the cranium was natural. The dura mater and its processes were converted into an osseo-cartilaginous substance, and the tunica arachnoidea was somewhat thickened and opaque. Pia mater healthy. The brain was much indurated, but the *cerebellum*, *medulla oblongata* and cerebral nerves were, on the contrary, soft and pulpy. The posterior cornua * of the lateral ventricles were carefully examined, and in each was found the elongation termed Hippocampus Minor. The spine, and its membranes and nerves, were free from any morbid appearance. The lungs and heart were in a healthy state. The small intestines were the only abdominal viscera which exhibited marks of disease. The inner membrane of the duodenum was highly vascular, whilst that of the jejunum and ileum, particularly near the termination of the latter gut, was beset with numerous sphacelated patches. The uterus and bladder were healthy.

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* Consult M. Pinel's Treatise "sur l'Alienation Mentale;" an excellent article by M. Esquirol, entitled "Hospice d'Aliénés," in the Dictionnaire des Sciences Medicales; and Georget's Essay "De la Folie."

ANTONIA —— ætat. 51, had been the subject of mental derangement for several years. Her memory was surprisingly strong and vigorous. She had a perfect recollection of the most trifling incidents which had occurred at a very early age, and could relate them with a minuteness and accuracy scarely to be credited. Her insanity was of a lively character, but her conduct was at all times orderly and peaceable. She complained during the last nine months of her life of severe pain in the hypogastric region, which was attended with a copious sanious discharge from the vagina. Her health gradually declined, and she expired in a state of extreme debility and emaciation. *Dissection.*—The encephalon was perfectly healthy, with the exception of the pineal gland † and its peduncles, which were considerably enlarged and indurated. The viscera of the thorax and abdomen were free from disease. The cervix uteri was found in a state of ulcerated carcinoma; and its inner membrane was studded with several scirrhous tumours of different sizes, which could not be detached from this viscus without lacerating a portion of its substance. Bladder sound.

MARGARET ——, ætat. 36, had been afflicted with insanity for the period of four years. It appeared, from the statement of her friends, that disappointment in a matrimonial alliance had been the cause of her derangement; and indeed her conversation had a constant reference to the object of her devoted attachment, and was at all times tender and affectionate. She occasionally enjoyed lucid intervals, during which her conduct was perfectly correct. This unfortunate female had for a length of time experienced considerable pain and difficulty in voiding her feces, which were scanty; and, at the time of her death, scarcely thicker than a common pencil. Blood was occasionally mixed with them; and the discharge from the gut, which before had been mucous, now became decidedly purulent. The complaint seemed to have been for a long time stationary; but its progress, during the last two months of the patient's life, was very rapid. Her constitution was quite exhausted; and death happily released her from a state of severe and continued suffering. *Dissection.*—On examining the brain, it was discovered that a deposition of serum in considerable quantity had taken place between the tunica arachnoidea and pia mater. The ventricles were nearly filled with fluid, and the *foramen monroianum* was much enlarged. The convolutions had become large and very distinct; but the cerebral mass presented no deviation from a healthy state. The lungs and heart were sound; and there was no perceptible disease in any of the abdominal viscera, except the rectum. The upper portion of this gut was in a state of scirrho-contraction, which, in the inferior portion, had degenerated into ulceration, and had partially extended to the vagina. The caliber of the bowel was so much diminished, that a small quill could not be introduced without considerable force. The sigmoid flexure of the colon was much dilated, and it contained a vast accumulation of fecal matter. Its coats also were harder and thicker than we find them in a natural state. The uterus was sound. The muscular fibres of the bladder were somewhat enlarged and indurated, but its inner membrane was healthy.

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* The posterior cornua were wanting in the heads of two idiots examined by Dr Hastings.

† Several cases of insanity are described by Morgagni, in which this gland was found enlarged.

Agnes ——, ætat. 16. This interesting young female was insane on one particular point.* She fancied herself no less a personage than a daughter of the King of France, and treated her medical and other attendants with a degree of hauteur, which her supposed rank seemed to authorize. Her natural disposition was proud and haughty. Occasionally she condescended to converse with strangers, when they approached her with becoming respect and humility; and she evinced considerable knowledge on several literary subjects, to which her attention had been directed in the course of a previous liberal education. Her general health had for some time suffered from great irregularity in the quantity, quality, and duration of the catamenia. She was seized suddenly with a severe attack of peripneumony, which terminated fatally in the course of a fortnight. *Dissection.*—No morbid appearances were observable in the brain. Each thoracic cavity contained a small quantity of serous fluid. The pleuræ costalis and pulmonalis of both sides of the chest were firmly conjoined. The right lung was completely hepatized; the left was very firm, compact, and turgid. The heart was sound. The abdominal and pelvic viscera presented no unusual appearances.

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* This form of mental hallucination is termed by the French "monomanie." It generally derives its character from the natural disposition of the patient. Amongst women, M. Georget says, "La vanite plutôt que l'orgueil porte les femmes à se faire reines ou princesses; elles ont plutôt en vue dans cet état moral la parure, que le pouvoir de commander."

Mary ——, ætat. 51, had long laboured under mental alienation. The character of her malady at its commencement was furious and vengeful; but for the last few years, it had been succeeded by a state of profound melancholy and panophobia. Her head was constantly under the bed clothes, and when they were removed for the purpose of administering food and medicine, she viewed every object which approached her with an eye of suspicion and distrust. She could not be prevailed upon, either by the most urgent entreaties or powerful threats, to utter a single word. She continued in this state until her death, which seemed to proceed rather from a gradual decay of the constitution than derangement of any particular organ. *Dissection.*—The brain was somewhat harder than natural, but in every other respect it was perfectly healthy. The thoracic organs were sound. The abdominal viscera appeared healthy; but the mucous membrane of the small intestines exhibited marks of inflammatory action, yet no disorganization in their structure had taken place. The bladder and uterus were free from disease.

In each of the above cases, we perceive a wide difference in the character of the mental hallucination. In the first, the natural affections were completely changed, and the intellects sustained a periodical loss of vigour; in the second, the patient had sunk into a state of helpless fatuity, being treated in every respect like a child; in the third, the faculties of reasoning and judgment were impaired, whilst that of memory had acquired increased strength, readiness, and susceptibility; in the fourth, the mind continued to brood upon that object which had last occupied its most frequent meditations when in a state of health; in the fifth, the reason was perverted on one particular point; and, in the last, mania had degenerated into profound melancholy. How are we to account for this diversity of mental aberration? Can it be explained by any peculiarity in the structure of the brain, or in its deviation from a healthy state? In one instance, a portion of its substance was found in a cartilaginous state; in another, the dura matter was diseased; in a third, the pineal gland was unusually large and firm; in a fourth, fluid was deposited in considerable quantity in the ventricles; whilst in two well marked cases of insanity, the texture of the cerebral mass presented no diseased appearances. Are we authorized in supposing, that the alteration observed in the organic structure of parts of the brain in four cases, was the cause of the mental derangement under which the patients laboured? If so, where are we to look for the cause of insanity in the other two? not in the brain, (although Dr Spurzheim says, "the cause of every derangement of the manifestations of the mind belongs to organic parts)" for it was perfectly sound. We meet, too, with similar morbid appearances after disorders of a very different kind, in which, the mind has to the last enjoyed the undisturbed exercise of its faculties. In idiotism, which is mostly congenital, examinations after death generally afford us evidence of diseased organization of the brain; but in other disorders of the mind, they frequently fail to detect any physical lesion or malformation. Mr Lawrence, who, from his connexion with Bethlem, must have very considerable experience in the pathology of mania, states, that in the great majority of cases which he has examined, the most obvious marks of disease were found in the brain; he, indeed, goes so far as to affirm, that he has scarcely ever met with a case of insanity in which the brain was found entirely sound. The results of his pathological investigations are at variance with those of Pinel, Esquirol, and Georget. The last named physician thus expresses himself respecting the morbid appearances which were observed in the examination of nearly 300 lunatics.* "Toutes les alterations que nous avons observées sur les aliénées de la Salpétriere sont consecutives au developement de la folie, excepte celles des cerveaux d'idiotes, qui sont primitives et liées à l'état intellectuel." The want of uniformity in the parts of the brain which are found diseased in particular mental affections, is one great cause of the obscurity in which the pathology of mania is at present enveloped.

VI.

Observations on Elephantiasis, as it appears in the Isle of France. By J. Kinnis, M. D. Communicated by Sir J. Macgrigor, Director-General of the Medical Department of the Army.

Sir,

While stationed in the Isle of France, I had opportunities of examining seven cases of the tubercular elephantiasis; and, as the history of that disease is still very imperfect, and can only be improved by descriptions of the phenomena it presents in the different countries in which it prevails, I take the liberty of laying before you the result of my observations.

In the patients I examined, the parts chiefly affected were the face, ears and extremities. The skin of the face was irregularly thickened, swollen and tuberculated; the tubercles being either small, elevated, hemispherical or large, flat, oval

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[*] See page 500 of his Treatise before named.

and irregular. They were generally large on the forehead, and separated from each other by deep furrows; smaller and more confluent on the cheeks, which sometimes hung down from the bones, stretching and depressing the corners of the mouth. The wings of the nose were enlarged, shapeless and unequal; each possessing its cluster of tubercles, with occasionally a strong one on the bridge. The lips, when affected, were penetrated by hard, whitish bodies, like recently formed cicatrices; the lobes of the ears were knobbed, thickened and enlarged; and, in one or two cases, the border of the helix was notched with small tubercles. The tubercles were smooth, glossy, shining, frequently confluent, sometimes on a level with, or only to be felt imbedded in, the skin, and varied from one or two lines to above an inch in diameter. In Mulattoes they were of a light, livid, copper colour; in Blacks, merely a shade deeper than the sound skin. Their cuticular lines, folds and depressions, were every where more distinct and larger than natural. The deeply wrinkled forehead, the bare swollen eyebrows, the heavy pendulous cheeks, the irregular expanded nose, the depressed mouth, and the thickened, elongated ear-lobes, combined to render singularly harsh and uncouth the whole visage, and to disguise the form and expression of every individual feature.

The internal surface of the mouth was generally more or less affected; the tubercles being either red, smooth, shining, about the size of a split-pea, and confined to the root of the tongue; or yellowish-red, flat, confluent, and occupying the greater part of the palate, uvula, fauces and tonsils; or stuck upon the hard palate, in the form of a single, large round bottom, with an irregular surface and depressed centre; or, lastly, particular parts of the lining membrane of the mouth were merely of a livid colour, without other change. In one case, the point of the uvula was tucked back, and appeared as if cut off until examined by a probe: in another, the uvula was relaxed and elongated nearly an inch; and, in the same case, the partition of the nostrils was ulcerated, and discharged a considerable quantity of matter: in a third case, one of the nostrils was blocked up with inspissated matter, apparently likewise from ulceration. Several of the patients were subject to nasal hemorrhage, and most of them had a hoarse nasal voice. They experienced no inconvenience from the presence of tubercles in the mouth, and one or two were not even aware of their existence previous to examination.

In one Mulattress, the trunk had a faintly mottled appearance, pale, copper-coloured spots being dispersed over it; in another, discoloration of the same kind was confined to the breasts: and, in a Mulattoe boy, there were numerous, small, distinct tubercles on the loins. In the remaining four cases, the whole skin of the neck and trunk was perfectly natural in colour and texture, perspired freely, and enjoyed unimpaired sensibility.

In one case, the scrotum was beset with small confluent shining tubercles.

The inside of the thighs and arms, and the flexures of the large joints, were seldom affected with the disease. On the back and outside of the former, and, on the calves of the legs, the skin was moveable over the subjacent muscles, but thickened, indurated, swollen, smooth, shining; and, either divided into gentle risings and depressions, or pervaded by flat clusters of tubercles, not sensibly elevated above its surface, or studded with distinct tubercles. The skin of the forearms, legs, and feet, was harsh, dry, scaly, or scabbed; in some parts adherent to the bones, in others loose, wrinkled, and tuberculated. It was particularly coarse and corrugated in the neighbourhood of the knee and elbow-joints, each of which was generally occupied by a cluster of tubercles, covered by a blackish grey scab. In one case, the skin of the legs, ankles and feet, was of a clay-brown colour, hard, thickened, and sulcated, or divided into numerous, irregular compartments, by deep fissures, from which oozed out occasionally an offensive discharge; in another were several large indolent sores on both legs.

The palms of the hands were seldom tuberculated, but had a dry, smooth, shrivelled appearance, as if the fat had been absorbed from under the skin. The backs of the hands, and more particularly of the fingers, were swollen, thickened, flabby, and beset with oblong tubercles, impeding the motion of the joints. The nails were coarse, fluted, and adunque. The feet were sometimes œdematous, but seldom much tuberculated. One patient had lost four toes of the right foot, excepting a single phalanx, which three of them still possessed; and another had lost two phalanges of the little finger; but these were the only examples of mutilation in any part of the body. In one case, the terminal bone of the great toe was exposed and dry; in another, there was a circumscribed, gangrenous spot, on the fourth toe; and, in most of the cases, there were open, indolent sores, on the backs of the fingers, the bend of the ankle joints, the soles of the feet, the points of, between, or under the toes; sometimes superficial, and of a red colour; at others foul, discharging little, surrounded with hard, irregular edges, or overgrown by morbid cuticle.

The parts affected with the disease were benumbed, or, as the patients sometimes expressed it, "asleep," but they had never entirely lost their sensibility.

The hair was always in natural quantity on the head; perhaps slightly diminished in the axilla, on the beard and pubes; rather thin and short on the edges of the eyelids; and almost, or entirely, wanting on the eyebrows.

The pulse was generally weak, and above a 100 in a minute. The appetite was good, tongue clean, and bowels regular.

In all the cases excepting one, the existence of the femoral tumour, discovered by Dr Adam in patients affected with elephantiasis in Madeira, was distinctly ascertained. It was occasionally found on one side only; but more generally in both, a few inches below Poupart's ligament, of an oblong form, and sometimes uneven surface (as if arising from the union of two enlarged glands), from two to three inches in length, when largest, and moveable, under the skin, as well as over the subjacent parts. At intervals, of from one to three or four months, these swellings were subject to attacks of acute inflammation, preceded by a fit of shivering, and accompanied with smart febrile symptoms, which lasted two or three days, and then subsided, leaving the tumour nearly as before. Sometimes the gland of one side, sometimes that of another suffered; but never both, during the same attack. In one of the patients, a fine Mulattoe boy, in his 13th year, there were two cicatrices in the left groin, and two others a little lower down, on the front of the thigh. The abscesses, of which these were the result, had begun to form about nine months before I saw him, had broken spontaneously, and discharged matter three months before they closed. Previously to their appearance, he had been subject to the usual attacks of fever and of inflammation, in one or other of the femoral tumours, (which still existed in both sides), but had been exempted from these attacks ever since.

The wasting of the genitals, described by Dr Adam, had not taken place in a single individual whom I examined; the testicles in males, and the breasts in females, being constantly of their natural size. With regard to the functions of these organs, neither the wonderful salacity ascribed to the miserable victims of this loathsome disease by some authors, nor the utter extinction of the venereal appetite, said to characterize them by others, existed in any case. One of the female patients, who had been affected with the disease only two years and a half, affirmed, that, though she had ceased to menstruate from its commencement, or to experience her former sexual propensities, she had yet suffered a miscarriage about 12 months before I saw her, and continued to cohabit with the person by whom she was kept. Another was the mother of two young children, one of whom I saw at her breast: she cohabited with a Black, but received him less frequently than before. The two remaining female patients had lived for some years in a state of celibacy; but they menstruated regularly, and both unequivocally affirmed that their sexual desires were not extinct. Of the male patients, one had not attained the age of puberty: another stated that he had abstained entirely from venereal pleasures for the preceding twelve months; assigning as his only reason a conviction, that such indulgences were sure to exasperate the disease. The last asserted that his sexual propensities were exactly the same as they had been previously to the commencement of the disease, and that he indulged them occasionally.

One of the patients, as has already been remarked, was in his 13th year, and the disease had existed in him six years. The ages of the other patients lay betwixt 23 and 50, and in none of them had the disease commenced before puberty. Three were Mozambique slaves, who had left their native country at an early period of life, and could give no satisfactory account of their parentage. The remaining four were coloured natives of the Isle of France. One of these had lost her mother in her youth, and never known her father; but had brothers and sisters in perfect health. The next two stood in the relation of mother and daughter. The husband of the former had been dead eight or nine years; he had long been afflicted with palsy and dropsy, to which, only two years before he died, was supperadded elephantiasis. Her daughter was attacked about the time of her husband's death; she herself, about two years after; and one of her sons had since fallen a victim to the disease. Her father was a Frenchman, her mother and maternal grandfather Creoles, and none of them was ever affected. The last patient appeared to have inherited the predisposition from the family of his maternal grandmother, who was never attacked herself, but who lost two sisters and three nieces by the disease. None of his other relations, for three generations back, were ever known to have been affected. I saw his mother, with three other children, in the best health. She and her mother were Creoles, her grandparents Europeans.

One of the patients could not tell how many years the disease had existed. Another had been affected two years and a half; all the rest from six to nine years.

It is currently believed in the Isle of France, that Diego Garcia possesses a specific cure for elephantiasis; and there is a law empowering the governor to transport thither persons labouring under that disease. This law was probably enacted for the protection of the community from a pestilent contagion (as it was once universally supposed to be), rather than with any hope of curing the sick. But whatever may have been its origin, it is now very rarely enforced. The cure is generally ascribed to the use of turtle, which is very abundant in Diego Garcia, and on which the patients are said not only to feed almost exclusively, but to drink the blood warm from the body of the dying animal. The cure is never permanent, the disease always appearing again soon after the patient's return to the Isle of France. *

There are several other diseases incident to the natives of tropical climates, which have been generally considered as more or less related to, and sometimes perhaps confounded with, the tubercular elephantiasis. Of five cases which fell under my observation in the Isle of France, one approached more nearly to the leuce, † as characterized by Dr Bateman,

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* This mode of treatment is directly opposed to the dietetic rules laid down by Dr Hillary in this disease. Patients, he says, " must religiously abstain from all swines flesh, and all fat meats, and every thing that is oily, fat, or greasy, either in sauces or otherways; and that not only during the time they are under this course of medicines, but for many years after." Diseases of Barbadoes, 2d ed. p. 332.

† Dr Bateman believed this to be the Hebrew leprosy—a disease of which Moses (Leviticus xiii.) describes six distinct forms, rendering the individuals affected unclean, and requiring seclusion from society. Three of these were confined to the scalp and beard (ver. 29 to 37, and 42 to 44), and three appear to have attacked indiscriminately any part of the skin. Of the forms last mentioned, a bright white colour was common to all the three; but the first was characterized by the spot affected being " deeper in sight than the skin," and having the natural colour of its hair changed to white (ver. 2, 3, and 18 to 20.); the second by the absence of these distinctive marks, by the spot or scab assuming in its progress a dark colour, and spreading, at a later period, " much abroad in the skin" (ver. 4 to 8, 18, 19, 21, 22, 26 to 28.); and the third by the white colour, extending over the whole skin from head to foot, and terminating in, or giving forth, excrescences of " raw flesh" (ver. 12 to 17.) It is evident that none of these modifications possesses the characters ascribed to leuce by Dr Bateman (Synopsis of Cutaneous Diseases, note on lepra alphoides), though the first agrees very closely with the account of that disease delivered by Celsus (de Medicina, lib. V. cap. 19.) If, however, the first form be referred to in verses 10, 11., and the first and second in verses 24 to 27, (of which, from the context, I think there can be no doubt), the advanced stage of the first and second, as well as of the third, was distinguished by excrescences of " quick raw flesh;" and, were the disease described by Celsus the same as any one of them, surely so striking a symptom would not have been omitted, even in his concise description. The occasional, if not invariable, occurrence of this symptom, during the progress of the disease, lends much probability to the opinion of those writers, who consider the Hebrew leprosy as identical with the yaws of modern times. Dr Good, on the contrary, describes it as a scaly disease, synonymous with the Grecian leprosy; and divides it into three varieties, corresponding with the three species into which vitiligo is divided by Celsus. In regarding the leuce as a scaly disease, denominated, in his Nosological System, *Lepriasis candida*, Dr Good differs from Dr Bateman; while he agrees with him in hinking it a variety of the Hebrew leprosy. See a learned disquisition on this ubject, in his Study of Medicine, Vol. IV. p. 574.

than to any other disease of which I remember to have seen an account. Two were probably examples of the "leprosy of the joints," described by Dr Hillary in his work on the Diseases of Barbadoes, and more lately by Mr Robinson (who proposes to call it *elephantiasis anaisthetos*), in the tenth volume of the Medico-Chirurgical Transactions. The remaining two were examples of the Barbadoes leg, or the Bucnemia tropica of Dr Good.

The first of these affections occurred in a Negress of Madagascar, thirty-five years of age. The disease consisted in large, circumscribed, irregular patches of discoloured and insensible skin, extending over nearly the whole body, the only parts exempted being the face, right fore-arms, and legs. The diseased was not perceptibly raised above the level of the healthy skin, but simply of a lighter or pale whitish colour. It might be pinched or pricked with impunity; but the patient complained of a most troublesome sensation of itching and prickling; and the friction employed to relieve this symptom occasionally brought out blisters, terminating in superficial ulcers, of which the scars were to be seen on the elbows, hips, and other parts of the limbs. These scars generally occupied parts indistinctly marked by the disease, though, on examining them attentively, discoloration could still be traced. There were two blisters on the palm of the hand. There were no tubercles on any part of the body, and no vestige of a swollen gland in either thigh. The hair was rather thin on the eyebrows; in natural quantity on other parts. Pulse 106 (in the erect posture, and perhaps agitated), appetite good, tongue clean, bowels regular. She had two healthy children, the first about nine, and the second about six years old. Her lover visited her occasionally, but she professed perfect indifference to sexual pleasures. The disease had existed about nine years. Her father had a single white patch on the buttock; her mother nothing of the kind. But for the insensibility with which the discoloration was accompanied, this case would have corresponded to the *epichrosis poecilia*, or pye-balled skin of Dr Good.

Of the two patients affected with the "leprosy of the joints," one had lost the great and small toe of the left foot, and either one or two phalanges of each thumb, and of every finger of both hands. Three of the fingers retained their nails, after the separation of their terminal bones.* The left foot was about one-fourth shorter than the right, and so much swollen that its bones could not be distinguished. The joints of several of the fingers were bent to right angles. Most of the mutilated parts were cicatrized; but there remained a minute ulcer, on the stump of the right thumb; another—deep, circular, offensive—under the ball of the great toe; and a third, about three inches in length, but clean and superficial, on the sole of the left foot. There were no tubercles on any part of the body; the skin was perfectly sensible, and of natural appearance, excepting on the toes, where it was harsh, dry, and hardened by cicatrices. Hair in every situation in proper quantity. Pulse 76, and very weak, appetite good, bowels regular. There was an evident fulness and an enlarged gland in the upper and inner part of the left thigh, and a very slight fulness also on the right. He was subject to attacks of pain and increased swelling of these glands, accompanied by febrile symptoms, and the breaking out afresh of the ulcers on his toes. His generative organs and functions appeared to be perfectly natural; but he had not been able to persuade any negress to cohabit with him for the preceding four years. The disease had existed before the English came to the Island (12 years.) He was a Mozambique black, 35 years old, and knew nothing about his relations.

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* In one of the cases of tubercular elephantiasis also, the nail of the great toe remained attached, in its perfect state, to the proximal, after the separation of the distal phalanx. From these facts, and from the mutilated parts being so often

The other case of this disease occurred also in a native of Mozambique, about 25 years of age. The patient had lost all the toes of both feet; the middle phalanx of the right fore, and of both little fingers; and either one or two of the terminal phalanges of every other finger, and of each thumb. The extremities of all the stumps were slightly enlarged, and most of them exhibited a distinct cicatrix, drawn a little forwards, probably by the action of the flexor muscles. On both thumbs, and on one or two of the fingers, there was a small, hard, horny projection, in place of a nail. The fingers, which had lost the middle phalanx, had their proper nail remaining; the deficient bones appeared to have been thrown off at their posterior surface; and the first and last phalanges were anchylosed nearly

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found cicatrized, may we not infer, that the death of the bone is the first pathological change which takes place in this disease; that ulceration of the soft parts is only a secondary action, instituted by nature to get rid of a foreign body; and that the restorative powers of the parts, though always too feeble to renew the bone, as in ordinary cases of necrosis, are in general sufficiently active to form a healthy cicatrix, unless another bone dies, and requires to be thrown off before this can be accomplished?

at right angles. Three small ulcers on the left foot, nearly covered by thickened cuticle, were discovered by their fetid discharge. There were a few minute *pimples* on the face; but no *tubercles* on any part of the body. The uvula was short and slender; on each side of the tongue near its root, there was a rough, irregular, slightly raised, circumscribed patch, resembling two tubercles coalescing. The skin of the extremities was thickened, indurated, scaly, dry and shrivelled, particularly about the knees and elbows, not adhering to the subjacent parts; and, on the back of the arms and thighs, exhibiting an appearance like numerous small cicatrices. On the hips and loins were several old scars, which he said were whip-marks. About three inches below Poupart's ligament, on each side, there was an oblong swelling, running down the inside of the thigh, about two inches long and one broad. Testicles rather small. He had sometimes, but very rarely, sexual desire. Hair in natural quantity; pulse 104, and very weak; had been twelve years in the island, and remembered nothing of his parents. As he had lost his fingers and toes at least seven years before I saw him, and had only three small indolent sores on one of his feet, this may be considered an example of recovery from the disease.

The discoloration and insensibility of the skin, described by Dr Hillary and Mr Robinson, as preceding the loss of the joints, existed in neither of these cases, at the time they were examined. On the other hand, these writers take no notice of the enlarged gland, discovered in them both, at the upper and inner part of the thigh. This enlarged gland, subject to periodical attacks of acute inflammation, accompanied by fever and aggravation of their respective symptoms, is common to the Tubercular elephantiasis, the Barbadoes leg, and the "Leprosy of the joints;" and furnishes, I believe, the only point of resemblance betwixt these three diseases.

The two cases of Barbadoes leg differed in no respect from that disease as usually described; and it is unnecessary to introduce them here.

I have the honour to be, Sir, your most obedient
humble servant,

J. Kinnis, M.D.

Fort Pitt, Chatham,
24th April, 1824.

To Sir James M'Grigor, Director General,
&c. &c. &c.

VII.

Anatomical Remarks on the Fasciae of the Groin, &c. By JAMES SYME, Fellow of the Royal College of Surgeons of London and Edinburgh, Lecturer on Anatomy in Edinburgh, &c.

NOTWITHSTANDING the great and deserved attention which has of late years been given to the department of Surgical Anatomy, concerned in the formation and cure of inguinal and femoral hernia, it seems to be the opinion of students, that no intelligible description of the parts in question has as yet been offered to them in writing. Indeed, I have often found, to my vexation, after labouring, apparently with success, to impress some correct and definite ideas of the matter in the way of demonstration, that all the ground which had been gained was lost in a night's time, by the perusal of some confused and complicated explanation.

In most of the treatises hitherto published on Inguinal Anatomy, there seems to be, in my humble judgment, one great imperfection, proceeding from a forgetfulness of the characters which belong to some of the parts described. For, although a *fascia* never terminates by a defined edge, but, on the contrary, always has its margin continued more or less gradually into the neighbouring parts, the fasciæ of the groin, have been described as distinct when adjacent to each other, and really united together,—while some points of their attachments have been carefully particularized and named.

Hence the proverbial variety of nomenclature employed in describing the structure of the groin; for every man who examines the fasciæ of this part with scrutinizing eyes, cannot but observe some circumstances—trifling enough perhaps,—in the manner of their connexions till then undescribed—and which therefore, by affording a claim to the privileges of a discoverer, seem to authorize him in naming anew, according to his own fancy, all the parts connected with that thus subjected to his authority. But students, not making the distinction between a difference of opinion and a difference of language, distrust the reality of a structure which they believe cannot be viewed in the same light by any two who dissect it, and therefore bestow little or none of their attention upon the matter; consoling themselves with the mistaken idea, that many good operators must be unacquainted with this portion of surgical anatomy; since, where so many disagree, few can be in the right. But here they fall into an error; for all good anatomists, and, of course, all good surgeons, though differing, perhaps, as to nomenclature, and, in some little points, as to structure, are thoroughly familiar with the anatomy of the groin; and are consequently enabled to operate here with the facility, despatch and safety, which distinguish the proceedings of those men who are acquainted with the machine whose disorders they undertake to repair.

I am sorry, therefore, that students should ever profess inability to understand so important a part of anatomy; still more to think that teachers should ever encourage them to pass it over as incomprehensible; and, most of all, that they should ever pretend to doubt the existence of fasciæ, or deny their title to attention.

The intention of this Essay is, not to publish new discoveries of my own, or criticise the value of those which belong to others, but merely to give a general idea of the inguinal fasciæ, which may assist students in obtaining an intimate acquaintance with them, and prevent those who are inclined to do so, whether owing to ignorance or less pardonable motives, from condemning the simple structure in question as complicated or incomprehensible.

There is no part of anatomical nomenclature more defective than that employed to describe the various expansions of condensed cellular membrane which we so often find separating the different organs of the body. The terms Fascia, Membrane, Aponeurosis, Tendinous expansions, &c. &c. are used here indiscriminately, and according to fancy,—the structure itself being uniformly of the same kind or nature, and different only in appearance.

The interjacent partitions alluded to are sometimes thick, at other times thin; at one part (*e. g.* fascia lata, covering the thigh on its outer side,) evidently displaying the crossing tendinous fibres which give the interwoven structure,—at another (*e. g.* same fascia covering the thigh on its inner side,) so thin and closely compacted as to seem destitute of fibres; but are still, in truth, always of a similar constitution, and ought, therefore, to be designated by some general title, to which the qualifying epithets of thick, thin, tendinous, membranous, and such like, might be prefixed when necessary, according to circumstances.

Were this done, much confusion and ambiguity would be at once avoided. I have, therefore, for some time past, employed the term *Fascia*, to express the *kind* of structure at present under consideration,—not as being in itself unobjectionable, but as being already applied to many of its particular portions throughout the body. As I shall take the liberty of doing the same in the following paper, it may be right to state more distinctly the characters of that structure which I understand by the term Fascia.

A fascia is an expansion of condensed cellular substance, variable in thickness and strength, but always composed of interwoven fibres. Its use is to defend organs, and confine their motions; for which purposes, besides possessing much strength in proportion to its thickness, in consequence of the crossing direction of its component fibres, it is always attached to bones when they are within reach, and also to muscles, which thus not only obtain secure points of insertion, but, moreover, are enabled to assist the fasciae in performing their duty of compression, by tightening them occasionally to a greater degree than that which usually belongs to them. Some muscles seem to have no other function than tightening the fasciæ, and are therefore termed *tensors*.

Students are often led into erroneous ideas of the fasciæ, by expressions contained in the common books of anatomy. We are told, for instance, that the use of the *fascia lumborum* is to afford an origin or unyielding point of attachment to the *latissimus dorsi.* But the *fascia lumborum* is an organ as independent as the *latissimus dorsi.* And if one *must* be regarded as subservient to the other, I should certainly say that it was the muscle; for the fascia is evidently intended to bind down the long muscles of the back, the *sacro-lumbalis* and *longissimus dorsi* namely,—in which duty it will be *assisted* by the *latissimus dorsi, serratus posticus inferior*, and other muscles attached to its edge, owing to the stretching and tightening effect which must result from their action.

It seems not less unreasonable to regard the tendinous fascia which covers the cranium, and lies between the integuments and pericranium, as *belonging* either to the occipital or frontal muscle, or as a middle tendon separating the two bodies of one muscle (the occipito-frontalis); the cranial fascia, just as that of the loins should be considered an independent organ, having a peculiar duty to perform, namely, defence of the subjacent part, that is the pericranium, which again protects the bone. And it is not difficult to understand how this fascia will act as a defence to the subjacent organs, since, being strong, tough, and resisting, and at the same time so loosely connected on both surfaces, as to be susceptible of a sliding motion, it must tend very much to diffuse and so weaken the effects of any violence offered to the head, especially when the direction in which it acts is not perpendicular to the bone, but more or less oblique.

In mentioning the characters of fascia, I may finally remark, that it never terminates by an abrupt well defined margin. It sometimes appears to do so; but in these cases it will always be found that the pretended margin is a fold, and that the real edge is connected with some other fascia, the periosteum, or, in fine, the common cellular substance; so that all the fasciae of the body thus appear to be continuous.

Having premised these general observations, I now proceed, not without much diffidence, to attempt, what so many have failed in accomplishing, an intelligible description of the fasciae concerned in hernia.

The *sacro-lumbalis* and *longissimus dorsi* muscles, while lying in the cavity which is formed by the lumbar vertebræ, are bound down into their place by a strong double fascia, named the *Fascia Lumborum*—whose outer portion is attached to the spinous, as its inner is to the transverse processes of the vertebræ just mentioned. At the external edge of the long muscles in question, the two divisions of fascia unite together, and thus form a very strong sheath. At this point of union, several muscles are attached to the fascia lumborum;* these are, the *latissimus dorsi*, *serratus posticus inferior*, *obliquus internus*, and *transversalis abdominis*. All of which must plainly, by their contraction, tighten the sheath which compress the long muscles of the back.

On the fore part of the abdomen, there are two muscles directly opposed to the *longissimus dorsi* and *sacro-lumbalis*, which, from the straight direction of their fibres, are named *recti abdominis*.† These have their displacement prevented after the same fashion as those of the back; with this difference, that there

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* Some people denominate the internal layer of the sheath, which, as just stated, is attached to the transverse processes of the lumbar vertebræ, *fascia transversalis*, and confine the meaning of *fascia lumborum* to that part which is fixed to the spinous processes; but this nomenclature certainly leads to confusion, as another portion of fascia is very generally designated as the transversalis.

† Many explanations have been offered, by way of accounting for the use of those transverse tendinous intersections, which we see crossing the recti muscles at three or more points of their extent, and forming a firm union with the sheath at its fore part; but most anatomists still profess inability to satisfy themselves or others on this subject. For a long while past it has appeared to me, that the intersections alluded to are useful, chiefly, besides preventing splitting of the muscle, by increasing the number of its fibres, and in adding to its contractile force.

If the muscles A and B be of the same size, it is clear that if B be divided across by the tendinous intersections *a*, *b* and *c*, the number of its fibres must be four times greater than that of A. And if the strength of a muscle be in proportion to the number of its fibres, which every one will allow, then the force of B must be four times greater than that of A. (See fig. I.)

I may take this opportunity of mentioning, that the use of the pyramidal musbeing no bone lying between them, the containing sheaths are formed entirely by fasciæ, the anterior and posterior portions of which unite together at each edge of the muscle. Between the two recti, their union extending in the mesial plane from sternum to pubes, is named *Linea Alba;* while that which subsists at their external edge is the *Linea Semilunaris,* so styled from its gently curved direction, which follows the shape of the muscles; and they, as every body knows, gradually diminish in breadth from sternum to pubes.

The double fascia, which forms the strong sheath of the recti muscles, being distinctly composed of interwoven tendinous fibres, may be entitled the *Fascia Tendinosa Abdominis.*

This fascia extends over the whole hypogastric region, that is, all the abdominal parietes below the level of a line drawn across from the superior anterior spinous process of one ilium to that of the other. Above this line its extent is strictly limited by the linea semilunaris.

There are several muscles attached to the fascia tendinosa abdominis at the linea semilunaris, just as there are to the corresponding points of the fascia lumborum. These are the *obliquus externus, obliquus internus,* and *transversalis.*

It thus appears that the abdominal and lumbar fasciæ are directly connected together by two muscles, namely, the internal oblique and transversalis. And if it be then farther recollected, that all the other muscles mentioned, namely, the external oblique, *latissimus dorsi* and *serratus posticus inferior,* are attached to one common point, I mean the thorax, it will be allowed that the lumbar and abdominal sheaths are connected very strictly, perhaps more so than is generally considered by anatomists and surgeons.

What I have hitherto been saying will be illustrated, and, I trust, rendered intelligible by the diagrams.

A, A lumbar vertebra.

B, The double lumbar fascia, which forms a sheath for the large dorsal muscles.

a and *b,* Its two portions attached to the transverse and spinous processes of the vertebra A.

C, The point at which the two layers of the fascia meet, and into which the following muscles are inserted.

D, Latissimus dorsi.

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cle seems to be not generally understood, if it is fair to judge from the descriptions contained in our common books of anatomy.

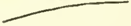
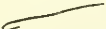
The little muscles in question rise from the pubes by a broad base, and are united with the fascia tendinosa abdominis, where its two portions meet to form the linea alba; their function is clearly and merely to tighten the sheath of the recti muscles.

E, Serratus posticus inferior.
F, Obliquus internus.
G, Transversalis.
H, Fascia tendinosa abdominis.
c d d, Its two divisions, which form a sheath for the rectus.
I, Linea alba.
K, Linea semilunaris, to which are attached,
L, The external oblique,
M, Internal oblique, and
N, Transversalis.

Having thus explained the connexions which subsist between the lumbar and abdominal sheaths, I shall now describe somewhat particularly that part of the fascia tendinosa abdominis, which extends from ilium to pubes, and is generally named its inferior margin.

It has been already stated, that this fascia is broadest in the hypogastric region, since it here not only covers the pubal attachment of the rectus muscle, but extends quite across to the superior anterior spinous process of the ilium; and being there also fixed to the bone, thus forms a sort of bridge over the brim of the pelvis—under which, vessels, nerves and muscles, going to the thigh, naturally pass; and viscera of the abdomen preternaturally protrude in cases of femoral hernia.

When the integuments and other parts covering the tendinous fascia have been dissected away, so as to expose it where extending between ilium and pubes, we see a whitish, dense, thick, ligamentous-looking margin, which is often considered a distinct ligament, and called, after Poupart, or Fallopius. More attentive examination, however, enables us to perceive, that this ligamentous appearance depends, just as the thick edge or hem of a towel does, upon a folding back of the edge upon itself.

This reduplication or folding back of the fascia does not exist to the same extent all the way from ilium to pubes; for about half the distance proceeding from the spinous process, its edge terminates simply thus,  ; it then begins to turn downwards and inwards upon itself, thus,  ; and continues to do so more and more until arrived at the tuberosity of the pubes and little spine, running backwards and outwards from it in the direction of the *linea ilio-pectinea*, where it is attached to the extent of half an inch or more, if the inverted edge, as sometimes happens, is of a greater breadth.

For the distance of about an inch and half from the pubes, the reflected border, or *Poupart's ligament*, is detached from the body of the fascia; and there is thus formed a sort of slit, which has been named the Inguinal Ring, and deserves great attention from the surgeon; since, besides allowing muscles, nerves and vessels, to pass between the abdomen and scrotum, just as the crural arch permits the exit and entrance of those belonging to the thigh, it also, like that aperture, occasionally becomes the seat of hernia, or an unnatural protrusion of the viscera.

To guard against this disagreeable occurrence, Nature has wisely provided, that whenever the abdominal viscera are strongly compressed by their muscular parietes, the external oblique muscle shall tighten the inguinal aperture by pulling the tendinous fascia so as to stretch the slit in its long direction, while, to prevent any enlargement of the ring by a rupture of the fascia, she has strengthened its iliac angle or commissure by crossing tendinous fibres; and that adjoining to the pubes by attaching the two portions of fascia firmly to the bone, and in such a way that the fibres of each are mutually interwoven.

Having thus explained the positions, &c. of the fascia tendinosa abdominis, we shall next proceed to the great femoral fascia.

When the integuments have been removed from the upper and fore part of the thigh, a quantity of cellular membrane, matted together, and enveloping many lymphatic glands with much fat, &c. may be seen filling up the superficial triangular sort of hollow which exists here between the *sartorius* and *adductor* muscles.

This cellular substance, like that in any other part of the body, may be divided into laminæ, the number and thickness of which will depend upon the dissector's patience and dexterity—a circumstance which is eagerly seized upon by those who, not understanding, wish to ridicule the fasciæ and so excuse their ignorance.

They triumphantly exhibit layer after layer of cellular substance dissected, so as to have the appearance of fascia; and then argue, that since *they* can *manufacture* as many fascias as they please, Nature cannot have formed any.

This loose confused congeries of glands, absorbents and fat, being taken away, we obtain a clear view of the fascia that covers all the inferior extremity. This strong and extensive fascia, though quite continuous throughout, has different appellations according to the situations in which it is found. Thus, the portion which we have at present to consider, sheathing the thigh, is named *fascia lata femoris.* The fascia lata on the external and posterior parts of the thigh is thick, strong, and obviously composed of interwoven tendinous fibres; on the inner parts it is thin and membranous. It surrounds all the muscles of the thigh, compresses them, and so renders their actions more energetic and effective. In the performance of this duty, it is assisted by several muscles, particularly the *glutæus* and *tensor vaginæ femoris*, which are inserted into it between two layers formed for their reception.

This fascia on the upper and inner part of the thigh covers the adductor muscles, and extends quite up to their pubal attachments. When resting upon the periosteum it becomes firmly connected with the brim of the pelvis, all the way between the insertion of Poupart's ligament, as above described, and the psoas magnus muscle, as it passes along with the iliacus internus into the thigh, and thus forms a floor under the great femoral vessels, as they descend from the pelvis. After running behind the vessels for two or three inches below the pubes, the fascia loses its thickness and compactness, adheres to them, and, in short, becomes continuous with their cellular coat or *sheath*, as it is called. Just when this change begins to happen, the vessels are covered by the fascia, which here seems anteriorly to terminate by a sharp well defined edge; but does not do so in fact, for the apparent edge is really a fold, the margin being reflected inwards and downwards, so as to disappear gradually in the sheath of the vessels on its fore part. A simple diagram will explain the matter fully.

The fold is continued upwards and inwards over the vessels in a sort of crooked or falciform direction, until reaching the pubal portion of the fascia lata, it there becomes attached, and so constitutes what is called the *falciform process of the fascia lata.*

When the integuments, with subjacent fat, cellular membrane and glands, are dissected off the lower part of the abdomen in a line with the fascia lata, a distinct and strong fascia continuous with it comes into view. This fascia being exterior to that formerly described as the tendinous abdominal fascia, from which it is separated by a layer of fat more or less thick in different individuals, has been named fascia superficialis abdominis. It is always well marked, and easily found. Care, however, must be taken not to destroy its connexion with the fascia lata, in dissecting it away rashly from the tendinous fascia. If the peritoneum be stripped from the abdominal parietes, an obvious fascia is displayed, thin and hardly distinguishable above the umbilicus, but strong and distinctly tendinous in the hypogastric region, especially below Poupart's ligament, where, like the fascia last described, it becomes continuous with the fascia lata of the thigh. It may now be perceived, that the abdominal muscles lie between two fasciæ, the superficialis abdominis, and this one—*the transversalis*, as it is named, or, as it might, perhaps with more advantage, be denominated the *profunda abdominis*. Finally, when the peritoneum is taken away from the surface of the pelvis and urinary bladder, a very strong tendinous fascia may be seen covering the muscles lying under the great vessels—lining the parietes of the pelvis—descending to its outlet, and there reflected upon the bladder. This is the *fascia iliaca;* and the part which covers the prostate gland, is no less than the famous *fascia prostatica*, which has been considered so important in lithotomy, and treated with so much respect by some writers on the surgical anatomy of the bladder.

I have now mentioned all the fasciæ concerned in forming the inguinal and femoral apertures. They are five in number, and may be arranged thus.

1. The fascia lata.
2. — iliaca.
3. — abdominis superficialis.
4. — — tendinosa.
5. — — transversalis.

Having now described the different fasciæ, I shall endeavour to explain the manner in which they are connected together between the ilium and pubes.

Though sections are little calculated for unravelling structure, they afford the best means of showing relative position. The appearances, moreover, exhibited by them, may be represented by simple lines, which of course are much less apt to mislead and confuse, than drawings that require attention to shading and perspective. I therefore resort to them on the present occasion, for assistance in making my meaning intelligible.

Let us suppose, first, that a section was made transversely through the union of the fasciæ at the point (*a*), about $2\frac{1}{2}$ inches from the spinous process of the ilium, their relative position would appear as in fig. IV.

The fascia lata here appears to split into laminæ which constitute the different abdominal fasciæ; and this description may be employed with advantage, provided the student be kept in mind that the expression *splitting* is used merely to facilitate the explanation of relative situation, and not to indicate the manner of formation, since the abdominal fasciæ are not less independent than the fascia lata, which might therefore, with equal propriety, be said to result from their union.

The edge of the fascia tendinosa not having at this point begun to turn back upon itself, ends simply between the superficial and transverse fasciæ. The fascia iliaca here seems to be continuous with that of the thigh; and indeed must plainly be so, since they are both at this part stretched over the same muscle, viz. the psoas magnus. Let us next suppose that the section was made at the point *b*, two inches distant from the pubes, and of course through the falciform process of the fascia lata, where it forms a border over the vessels, the appearances which would then be seen might be represented as in fig. V.

We here see the fascia lata and iliaca becoming continuous over the ramus of the pubes, where it bounds the pelvis. The lower margin of the fascia tendinosa has now turned upon itself, so as to form what is called Poupart's ligament; and the reverted edge is plainly identified with the fascia transversalis. I am aware that eminent anatomists deny the existence of this communication; but no authority, however great, should be trusted to in questions which can be decided by an appeal to Nature.

I have now only to speak of the mode in which the different fasciæ composing the crural arch—as the bridge over the vessels is named—are fastened to the pubes; and this is an interesting subject, as it has occasioned long and obstinate contentions. It must be owned, however, that the dispute here has been more about words than matters of fact.

The best method of demonstrating this piece of anatomy is to remove every thing which covers the crural arch—skin, fat, fascia superficialis, and fascia tendinosa—all but its inverted border, which helps to constitute the arch. We may now see that the broad extremity of Poupart's ligament is attached obliquely to the tuberosity and crest of the pubes; that the falciform process of the fascia lata, after passing over the vessels, is fixed to its own *pubal part*, and then becomes attached to the pubes all the way backwards in the direction of the *linea iliopectinea*, until it passes some way (about half an inch generally) beyond the insertion of Poupart's ligament, with the posterior edge of which it is strictly incorporated. It thus appears that the border of fascia extended over the vessels is not narrow or linear as many people think, but more than an inch in breadth.

This insertion of the crural arch into the crest of the pubes used formerly to be denominated Gimbernat's ligament; but it is now more generally named the crescentic portion of the crural arch. Indeed, Gimbernat has clearly no title to the honour of standing namefather to the part in question, as he described merely the attachment of Poupart's ligament.

I think that this description of the inguinal fasciæ will be intelligible to all who have dissected the groin. Description alone, of course, cannot afford clear ideas of anatomical structure; but it may be of practical service in directing the operations of the dissector, and enabling him to retain the clear ideas, which cannot be obtained without much manual labour.

The fasciæ of the groin serve as a sort of skeleton to it, and render it easy for any one acquainted with them to understand the relative situations and connection of the various important organs which belong to this part. They also, as already stated, constitute the aperture through which vessels naturally pass, and viscera preternaturally protrude, and are therefore immediately concerned in the different sorts of hernia.

Mr Geoghegan says, in a late Number of the Edinburgh Medical and Surgical Journal, that the fasciæ have occupied much attention unprofitably, since they are merely passive in the strangulations of hernia, and must be divided when the disease is remedied by operation, where the stricture is felt to offer most resistance. But I would ask Mr Geoghegan, if the stricture in femoral hernia, which in this case is formed by the innermost portion of the crescentic attachment of the crural arch, is not often so tight as to render its division extremely difficult?—if there is not an artery which may be wounded in the division of the crescentic portion?—and, finally, if Mr Dupuytren, the first of living surgeons—to say nothing of many other great though less justly celebrated names—does not relieve the stricture which opposes the reduction of femoral hernia, by dividing the falciform fold of the fascia lata?

But even granting that it may be possible for a surgeon, by dint of cautious, anxious groping, with trembling hands—as agitated and uncertain as his mind—to reach the stricture and divide it, without understanding the fasciæ which form it, I still think a knowledge of them is not to be despised, in as much as no man, however careless of his own reputation or patient's safety, can cut expeditiously and smoothly among parts with which he is not familiarly acquainted. And I think it should never be forgotten, either by surgeons or their patients, that the success of every operation, from the opening of a vein to the removal of a limb, *depends* less upon the *thing* that is done, than upon the *manner* in which it is performed.

VIII.

A Case of Hemorrhage into the Cavity of the Uterus, prior to Delivery. By WILLIAM HENDERSON CROWFOOT, Esq. Surgeon, Beccles, Sussex.

ABOUT noon, on the 22d of last February, the Honourable Mrs M., who had just completed the seventh month of her pregnancy, was suddenly seized with extreme faintness whilst quietly sitting in her breakfast room. The faintness soon increased to syncope, from which she was with some difficulty recovered, and which recurred upon the slightest approach to an upright position.

On the first alarm, a respectable practitioner in her immediate neighbourhood was sent for, who prescribed some cordials, and left her somewhat better. The most deathlike faintness, however, soon returned, when the same gentleman was again summoned, and a messenger was despatched for me. Upon my arrival, at about half past six in the evening, I found the patient with an extremely feeble pulse, a very pallid countenance, and an inability to rise from a recumbent posture without fainting. There had been no uterine discharge of any kind, nor any one apparent circumstance to account for this state of exhaustion. The movements of the child had been very strong and distinct early in the morning, but they had not been felt since the faintness came on. The patient complained of a most distressing sense of distension at the lower part of the body, but she had no pains resembling those of labour. In this perplexing state she continued, with little alteration, till about ten o'clock at night, when she expressed a belief that her labour was approaching. Soon afterwards a sudden and copious discharge of a slightly coloured fluid took place from the uterus, which she, as well as those about her, considered to be the waters of the amnion. On examination at this time, *per vaginam*, the *os uteri* was found scarcely at all dilated. Externally, the child could be very distinctly felt under the parietes of the abdomen on the right side, and, on the left, a tumour of equal bulk, but of a soft and compressible character. The patient's sense of distension was much relieved by the discharge of the fluid. During the night there were occasional slight pains, attended with more or less of hemorrhage, which latter, by six o'clock in the morning, had increased so much as to render it necessary to hasten the delivery of the patient. There had been so little uterine action, that, even at this time, it was with some difficulty I was enabled to ascertain that the membranes were still entire;—these I immediately ruptured, and found the head of the child presenting. The hemorrhage ceased on the discharge of the waters, expulsive pains very soon took place, and, at about nine o'clock, a recently dead child was born, and was immediately followed by a very large quantity of coagulated blood. The placenta was found loose in the uterus, and was at once removed. The uterus contracted readily, and there was little subsequent hemorrhage. The patient, naturally florid, long retained her pallid and almost bloodless appearance, but a good constitution enabled her shortly to recover from the effects of so severe a loss.

The sudden and alarming faintness, which occurred in this case, appears to have been occasioned by a formidable hemorrhage into the cavity of the uterus consequent on the separation, from some unknown cause, of a considerable portion of the placenta; whilst the closed state of the os uteri, by preventing the escape of the blood, restrained the hemorrhage, and prevented its going on to a fatal extent. After a time, the over-distended organ slightly contracted on its contents, and expelled, what I imagine to have been, the serous portion of the extravasated blood, whilst the crassamentum formed the elastic tumour on the left side of the abdomen. After the *os uteri* had begun to dilate, a gradually increasing hemorrhage took place, and rendered it necessary to rupture the membranes, and evacuate the waters, for the purpose of inviting a more efficient action in the uterus, which happily soon followed, and terminated a very anxious and perplexing case, in which a nearly fatal hemorrhage had taken place, without having been indicated by any external discharge.

Since the publication of Dr Gooch's paper, (12th vol. of the Medico-Chirurgical Transactions), on the Hemorrhage which sometimes follows the Expulsion of the Placenta and the Contraction of the Uterus, I have had an opportunity, in three cases, of witnessing the good effects of the preventive treatment which he recommends.

Mrs G., in three previous labours, had suffered from a frightful hemorrhage, which followed the expulsion of the placenta, after an interval, varying in the different labours, of from five to twenty minutes, notwithstanding the uterus had actively contracted. This patient having been again pregnant, I determined to try strict antiphlogistic treatment for five or six weeks prior to her confinement. The consequence was, that she altogether escaped the hemorrhage.

Mrs A., a lady of sanguine temperament, but delicate constitution, in her two first labours had rather more than the usual discharge after the removal of the placenta. In her third labour, the placenta was expelled as usual, the uterus actively contracted, and no hemorrhage followed for at least twenty minutes, when a most appalling one burst forth, and was with difficulty restrained by the most prompt and continued treatment. In her fourth pregnancy, the antiphlogistic treatment was pursued as in the former case, and with the same satisfactory result.

In another patient, Mrs. L., similar hemorrhages had occurred in two previous labours; in a third pregnancy, the antiphlogistic treatment prevented this inconvenience in the following labour.

In none of these cases did the hemorrhage immediately follow the expulsion of the placenta; an interval of from five to twenty minutes intervened; and I therefore infer, that a degree of relaxation of the uterus preceded the hemorrhage, and was as essential as the existence of the phlogistic diathesis. We find that the only remedies upon which we can depend in these cases, are such as have a tendency to produce firmer contraction of the organ;—the sudden application of cold to the abdomen, steady pressure externally on the uterus, and the introduction of the hand into its cavity. Dr Gooch found that *Le Roux's* remedy could not be relied on; and the case which I detailed first, in which alarming hemorrhage took place, into an uterus distended by a seven months' fœtus and its appendages, would indispose one to trust to the plugging the vagina as a means of restraining a bleeding into a dilatable organ.

IX.

Cases of Aortic Aneurism. By T. Proudfoot, M. D. Kendal.

Aneurisms have generally been divided into true and false; and although a disposition to drop this division has lately been shown, there still appears a necessity to maintain some distinction, in order that the diversity of morbid structure may be better understood.

A true aneurism is understood to be a dilatation of all the coats of an artery. This dilatation may be either of the whole circumference, or of a part of the circumference of an artery. A false aneurism is alluded to, when the internal and muscular coats have given way, and the cellular coat has become elevated into a cyst, or distended with blood; this case, of necessity, only occupies a part of an artery. A third species of aneurism, termed the Mixed, is when the spurious aneurism becomes ingrafted upon the true aneurism, or, in other words, when the artery first becomes dilated, and afterwards a rupture of its internal and middle coats takes place, with a consequent effusion of blood into the cellular sheath, or cellular substance, which invests the artery. All this is quite arbitrary, and appears not very natural. It must be allowed, that an aneurism is more genuine, when there has been rupture and effusion, than when there has only been simple dilatation. Why, then, call dilatation a true aneurism, while a greater lesion, or deviation from the natural structure, namely, a rupture of the two internal coats and dilatation of the cellular, is called a false aneurism?—and why call that a mixed aneurism, which is so naturally presented to the mind by dilatation, rupture, effusion, and distension? Perhaps this is the most frequent manner in which aneurism takes place.

The most distinguished writer on this subject in modern times was the late Professor Scarpa. The character he particularly contended for, is, that an aneurism is formed, in every instance, by the rupture of the internal and middle coats of the artery; and that the aneurismal sac is never formed by the dilatation of those coats, but by the external or cellular coat. He endeavoured to prove this, by observing, that the aneurismal tumour was always expanded to the side of the artery, and never included its whole circumference; and that the canal of the artery and aneurismal sac were always found separated by a septum, which was formed by the two internal coats of the artery; and in which might be found a fissure, which had allowed the blood to escape, so as to distend the cellular substance. He states, that the cellular substance being elevated by the effused blood, at first in the manner of an ecchymosis, then distended and compressed, acquired that degree of density and of additional hardness, as it had been found by the proper coats of the artery, prodigiously relaxed, distended, and thickened. Scarpa did not altogether deny, that, by relaxation and distension, an artery might become liable to rupture, only that it was not essential to the formation of an aneurism; and he laboured to inculcate, that there was a wide and essential difference between an aneurism, which was a rupture and effusion, appearing as an appendage or tuberosity, attached only to one side of the vessel, and a dilatation which occurred all round the circumference of an artery. But it would appear that he was unable to get rid of the results obtained by some of his predecessors; more particularly, perhaps, of the cases recorded by Morgagni; for Scarpa afterwards admits, that although the two affections are essentially different, they occasionally co-exist, particularly at the origin of the aorta.

By those who have contended for a difference between aneurism and dilatation, another distinction has been mentioned, namely, that concentrical laminæ, or lamellated concretions, as they have been termed, are generally found in aneurisms, when the internal and muscular coats have been ruptured, but never in a general or partial dilatation of the coats of an artery. This, however, seems very doubtful.

The views of Mr Burns of Glasgow, concerning thoracic aneurism were not very different from the concessions of Scarpa. He, however, clearly demonstrated, that an aneurism of the aorta, from its origin to beyond the curvature, existed in a preparation in his own possession. The whole circumference was dilated equally, and laminæ of coagulated lymph were found investing its internal surface. The internal coats were diseased; and rents or fissures could easily be perceived between the separated rings of the fibrous coat. Mr Burns farther maintained, that, in ordinary cases of aneurism, the first part of the morbid process was not a rupture of the internal and muscular coats of an artery, but "that the external coat has, all round the diseased part of the proper coats of the vessel, been fixed to the healthy coats more firmly than usual; and also that the texture of the cellular coat has suffered a change, for now it is denser and thicker than natural."

Mr Hodgson has, in a great measure, set the question at rest concerning the formation and structure of aneurismal sacs, by conceding that many aneurisms are formed by rupture of the internal and muscular coats of the artery, and dilatation of the cellular coat into a cyst, &c.; while aneurisms at the origin of the aorta are "generally formed by dilatation of the vessel;" he also admits, that partial dilatations frequently precede the formation of aneurism in the trunk and in the extremities.

Corvisart maintained, that aneurisms, far from being formed from within, outwards, by dilatation, degeneration of the internal coats, or rupture, are, on the contrary, formed from without, inwards, by erosion of the coats, and that the cyst is formed by the contiguous parts. Although this doctrine has obtained little credit, it seems difficult to deny that a disease resembling aneurism, may not occasionally take place, in the manner he has stated. I remember to have inspected the body of a soldier at Tarragona, who was supposed to have died of a protracted dysenteric affection, when, along with ulceration of the large intestines, and general disease in the abdomen, a large curdy mass was found embracing the abdominal aorta, communicating with it, and containing coagulated blood, which had evidently escaped from the canal of the artery, by erosion of the coats, from without, inwards. I proceed to recite the cases which gave rise to the foregoing remarks.

Christopher Workman, aged 39, was seized about two years ago, with a pain in his back, which was supposed to be rheumatic, and for which he underwent a course of treatment by the direction of a medical practitioner; but the pain in the region of the back still continuing, accompanied with general ill-health, he was induced to apply to the surgeon at the dispensary. When I first saw him, he had a sallow unhealthy appearance, accompanied with pain in the back, and a sense of weight and uneasiness at the præcordia, particularly after dinner. The tongue was covered with a white fur, and the bowels were costive; the skin was moist, and the pulse regular. As he resided in a very confined house, in a low, damp, unhealthy part of the town, I was led to consider it one of those cases of derangement of the organs of digestion, produced by a combination of causes, such as close application to a sedentary employment, bad air, poor living, and despondency. Treatment was commenced, by prescribing a few doses of cathartic medicine, a warm bath, &c. and afterwards antimonial powder, and a small proportion of submuriate of mercury was ordered to be taken every night, with the view of ameliorating the secretions of the digestive organs, and of curing or relieving the supposed rheumatic affection. Under this treatment, his general health improved materially, and the pain in the back was considerably alleviated. He was intrusted to the care of the surgeon, without any particular observation, and I heard no more of him for some time. By and by, however, I was again requested to meet the surgeon in consultation, who stated that he had not been able to cure Workman entirely, and that he believed the patient had been consulting every person he could meet with. He informed me, that, since I last saw him, he had tried a variety of medicines without avail. He said that the only thing which had given him any relief, was a combination of gin and sulphur. I dissuaded him from taking ardent spirits, but approved of a farther use of the sulphur, since it afforded relief. His appearance at this time was extremely sallow, and he had become much thinner, otherwise he was nearly as when I first saw him. His back was most minutely examined, both by the surgeon and myself, but we could discover no disease of the spine, which, we were led to suspect, from the constipated state of the bowels, and from a peculiar sensation in his limbs, which he would not let us call a weakness or want of power; he said it was a curious benumbed feeling, recurring at intervals. He was bled locally; had blisters repeatedly applied, and occasionally used strong stimulating embrocations; and under a course of his former medicine, which, from a trial of various others, was found to agree best with him, aided by cathartics at proper intervals, such as infusion of senna, with sulphate of magnesia, or compound colocynth pill, he derived great benefit; and my visits were for some time discontinued.

Three or four months afterwards, I was informed that he was as bad as ever, and that the alterative doses of mercury and antimonial powder, failed in procuring that relief which they had formerly afforded him. On this occasion he appeared much altered for the worse, his countenance had even a more sickly and unhealthy aspect than before; he complained of excruciating pain in the small of the back, and in the region of the left kidney, shooting along the course of the ureter of the left side, and down the thigh. He had some difficulty in voiding his urine, which he stated had lately been passed only in small portions at a time, and the quantity made during the twenty-four hours appeared to be much less than formerly. The urine was thick and high-coloured, depositing, after standing some time, a copious red sediment. The bowels had been kept open by the use of cathartic medicine; the tongue was clean, and the pulse regular, though somewhat accelerated. The affection of the lower extremities had rather increased, and indicated disease of the spine more unequivocally than at any former period; but still it was obvious that it was not simply a case of disease in the spine, and it was conjectured that the spinal affection appeared complicated, either with a calculous complaint, or some other disease of the kidney. The probability of an aneurism of the abdominal aorta, of lumbar abscess, or organic disease and enlargement of the spleen, pancreas, or supra-renal glands, or of the mesentery, &c., was constantly kept in view; but the most minute examination, both of the spine and abdomen, in the erect and in the recumbent position, did not enable us to discover any thing unusual, or to satisfy our minds precisely as to the nature of the disease. * The patient acknowledged that he had occasionally

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* I remember Dr Colles, professor of anatomy in Dublin, mentioning a very simple method by which he had, in some instances, been enabled to determine the nature of some doubtful cases of pulsation in the abdomen. He placed the patient on the back, slightly elevating the chest, and directing the thighs to be bent towards the trunk, so as to relax the fasciæ, abdominal, internal iliac, and psoas muscles, as

an increased pulsation of the heart, particularly after exercise, or when suddenly excited by casual occurrences: but this was only extorted by inquiry. The pulsation had not been of such a nature as to engage his spontaneous attention; and as the pulse gave no indication of aneurism, the circumstance could readily be accounted for by sympathy. At this period he tried the warm bath again, along with lithontriptic medicines and the usual cathartics, from which he derived considerable relief. The pain in the region of the left kidney, and along the course of the ureter, was very considerably relieved: his appetite improved also; and although the pain in the back still continued, it was not quite so severe as formerly.

In this improved state he continued for some time, when a few benevolent friends afforded him the means of going to Buxton, in June last year, where he resided for five weeks, making use of the bath and water by the advice and direction of a resident practitioner. He returned to Kendal greatly improved in health and appearance. The only thing he complained of was a slight pain in the back; but he had gained flesh and strength; he had no pain in the region of the kidney of the left side, or along the course of the ureter; the skin was moist, the tongue clean, the pulse regular, and the appetite good. I heard no more of him for several weeks, when I was again informed that he was not quite so well as when he returned from Buxton. On visiting him, I found that the pain in the back began to get worse about a fortnight after his return; but despairing of ultimate relief from medicine, he did not apply till the pain had become almost insufferable; he compared it to the introduction of a red hot knife into the back bone. The affection of the lower extremities had not increased, and the pain in the kidney and along the ureter had not returned. The secretion of urine was scanty, but it was voided without difficulty; the pulse was about 100, but regular; the bowels were never open but by the aid of cathartic medicine. The occasional use of the warm bath, fomentations, cathartics and anodynes, with perfect rest, comprised the principal part of his treatment about this period. Early in the month of No-

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in cases where the reduction of inguinal hernia is attempted. He then placed the points of one or two fingers, or of the thumb (according to the size of the patient), of the left hand, on the course of the abdominal aorta, with a view to cover it, while with the fore-finger of the right hand, he examined on each side of the thumb or fingers. If the pulsation was distinctly felt on either side, most frequently the case proved to be an aneurism, and *vice versa*. The heaving of a tumour, situated over the course of the aorta, was different, he maintained, from the pulsation in an aneurismal tumour.

vember, the pulsation in the region of the heart, and along the abdominal aorta, became more apparent; and although the pulse was regular, it now appeared probable that the disease under which the patient laboured would prove to be aneurism. The patient's sufferings from extreme pain became distressing even to the attendants. He was able to obtain ease only from increased doses of anodynes, assisted by a particular position, which was by resting on the knees with his hands under the forehead, or covering the ears, and with pillows placed under his body. Occasionally he changed this position, and kneeled down, resting his head on the bed, or on a chair or table, as in a supplicating position. Towards the end of November, a pulsating tumour appeared about two inches to the left of the inferior dorsal vertebra, and the same day increased to the size orange. This circumstance determined the nature of the case, and enabled us to give a decided prognosis to the wife and family of this poor man, who, to the last, bore his illness with much patience and fortitude.

The tumour did not enlarge. He gradually became weaker, and suddenly expired on the 14th of March 1824, two years and eight months from the commencement of his illness, and 16 weeks from the appearance of the pulsating tumour on the back.

The inspection of the body took place 36 hours after death by candle light, which is a great disadvantage to the minute inspection of morbid structure. In the cavity of the thorax, the left side appeared completely filled with coagulated blood. About 3 pounds of this extravasated blood, including the serum, was removed into a basin. Little blood was found in the left ventricle of the heart, or in the aorta. The heart and pericardium appeared perfectly sound, without previous marks of inflammation, ossification, or dilatation. The mitral and semilunar valves were also sound and natural. The right cavity of the thorax contained the usual quantity of moisture; and the right lung appeared remarkably sound. The only deviation from this was an unusual degree of paleness; but there were no marks of previous inflammation, or adhesion of the pleura of the lung, to that of the chest. Upon cutting into the substance of the lung, it was found unaltered in structure. The left lobe of the lung did not appear quite so sound, and bore marks of having undergone the process of inflammation at some former period. Upon examining the parenchymatous substance, there appeared a thickening and consolidation of part of the great lobe; but we discovered nothing like abscess, tubercle, or earthy concretion. We then began to trace the course of the ascending aorta, and were surprised to find it perfectly sound at the arch, and till it began to approach the vertebræ of the back, when a slight thickening of the coats on one side was observed; but still the artery appeared pretty sound, till about three inches before where it passes the *crura* of the diaphragm, where we discovered a large irregular aneurismal tumour. It appeared, when viewed anteriorly, not unlike a curdy scrophulous mass. There was found a chink, or fissure, through which the blood had escaped into the cavity of the thorax.

By farther opening the aneurismal sac, and introducing the finger, it was found to advance towards the right side of the thorax, and to include two of the vertebræ, which were denuded and carious; the intervertebral substance seemed to have suffered less than the bony part of the spinal column,—its form and structure remaining entire. The dorsal extremities of two of the ribs were denuded.

The abdominal viscera were sound, except the liver, which seemed to have suffered recently from inflammation, particularly of the peritoneal covering; it adhered generally to the peritoneum covering the false ribs, and a thick layer of coagulable lymph was found in those places where adhesion had not taken place. The kidneys, contrary to expectation, were quite healthy.

The inspection of this aneurismal sac would in some measure tend to confirm the theory of the formation of aneurism, so strenuously contended for by Professor Scarpa, namely, that the aneurismal sac is invariably formed by the dilatation and condensation of the cellular sheath, which invests the artery, and connects it with the contiguous parts, the internal and muscular coats having previously been ruptured. In this instance, the opinion is supported by the aneurism being large and irregular in figure. It is obvious, too, that the coats of the artery could not be dilated, so as to advance in front of the vertebræ of the back, towards the right side of the thorax, and,'in consequence of the dilatation of the posterior part of the thoracic aorta, find their way between the ribs and dorsal muscles, while the structure of the left half of the canal of the aorta remained in a healthy state. So far this case corresponds with the notions of Scarpa. But it is equally manifest, that there had been a great dilatation of all the coats of the artery; and it appears probable, that the proper coats of the artery formed the aneurismal sac, some time previous to the period when the tumour appeared externally near the spine.

From the nature and apparent progress of this aneurism, it seems to have been successively bounded by three or four different kinds of aneurismal sacs. In the *first* place, by the proper dilated coats of the artery. *Secondly*, After the internal and muscular coats gave way, by the cellular sheath of the artery. *Thirdly*, The cellular sheath was so blended with, and lost in the pleura, and the pleura was so thickened and altered by disease, that it formed the true sac of the aneurism anteriorly, through which the blood had escaped by a fissure, into the left cavity of the thorax. *Fourthly*, That portion of the aneurism which made its way to the back, was included only in a sac formed by the common cellular substance, or was simply an effusion into that texture. The internal surface of a great part of the sac so exactly corresponded with the colour and texture of the sound artery, and so plainly appeared a continuation of that membrane, that had any one attempted to persuade a person who had previously known nothing of the matter, that it was wholly of a different nature, he could not have failed to excite a suspicion, that he wished to inculcate what was contrary alike to common sense and to ocular inspection. Had this patient died twelve months sooner, it is very probable that a large aneurism would have been found lined by the proper dilated coats of the thoracic aorta. In another respect, this aneurismal sac corresponded with the doctrine of Scarpa. It appeared as an appendix, or tuberosity, and only included about half the circumference of the artery.

One of the most remarkable circumstances in the case of this patient, was the relief obtained from the use of medicine, and the great improvement in his health from the short residence at Buxton.

It is reasonable to suppose, that the vigorous and healthy circulation of the blood, beyond the seat of the aneurism, must have been interrupted or impaired; and that there must have been a consequent hebetude and imperfection in the action and secretion of all the abdominal viscera. The pressure of the tumour upon the nerves and spine would contribute to the same effect. The medicine which was used excited action, and promoted secretion, which it is likely would have been less perfectly performed without its use.

The inflected position of the body in which this patient obtained a temporary relief from pain, has been noticed and commented on, in similar cases of aneurism, by Morgagni, Malpighi, and other writers. In one case of aneurism of the aorta, Morgagni ascribed the relief obtained from inflection of the body, to taking off pressure from the trachea; and, in a case of enlargement of the heart, to the removal of pressure from the lungs. But, as it is frequently observed in other diseases besides aneurism, it seems more probable that this bent position, so eagerly and almost universally sought for, depends upon some more general law of the constitution;—it may be that in which the greatest number of muscles and organs are relaxed, and remain quiescent. The closing of the eyelids, and placing the hands on the ears, frequently observed in cases of extreme suffering, seem to be an involuntary effort to avoid the irritation of light and sound. The pain in the region of the left kidney may have been owing to the pressure of the tumour on the intercostal nerve, by which the kidney is partly supplied.

The widow of this patient ascribed her husband's illness to grief and anxiety. Passions of the mind are well known to exert an influence on the circulation of the blood, particularly in the receptacles of the chest, in exciting sudden and preternatural contraction and dilatation. By Morgagni and others, this has been cited as a reason, why the lower animals, that are not liable to numerous passions and emotions, are very rarely found affected with aneurism, while it is frequently met with in the human subject. The observation may be partly correct; but something ought, perhaps, to be ascribed to the greater duration of human life, and to the greater attention with which morbid changes are examined in the human subject than in brutes.

A singular case of thoracic aneurism occurred in a soldier of the name of Armstrong, belonging to the 27th regiment, while quartered at Cavan in 1816. When out on a "still hunting party" in the night-time, a species of fatigue-duty well known to the military serving in Ireland, he was suddenly seized with acute pain in the left breast, immediately after leaping over a ditch, and, as he expressed himself, felt as if something had given way in his inside. He was unable to proceed with the party, and was left in a cabin in the neighbourhood, till their return. On reaching the quarters of the detachment on the following day, he felt weak and languid, and appeared pale; but after relating the manner in which his indisposition arose, stated that he felt better, and that there now only remained a sense of weight and soreness in the chest, accompanied with a little thirst and loss of appetite. The pulse in both wrists was about 90 in a minute, and, though not irregular, had a peculiar fluttering sensation; sufficient even then, along with the other circumstances, to excite a suspicion as to the nature of the accident which had occurred. He was largely bled, put upon low diet, got aperient medicine at proper intervals, and was strictly enjoined to avoid every exertion either of mind or body, likely to quicken the circulation. He soon experienced the benefit of this treatment, and, anxious to return to his duty, wished to persuade me that he was quite well. In consequence, however, of an unusual pulsation about the region of the heart, he was detained a few days longer. About two weeks from his admission into hospital, a tumour appeared occupying the sternal extremities of the third and fourth true ribs, and suddenly increased to the size of a large orange, accompanied with evident pulsation, or rather a jet of blood, into the tumour. At this period there was every probability that the aneurism would soon prove fatal, and the patient was duly apprised of it. Nothing was omitted that was deemed likely to prolong existence, such as bleeding, low diet, perfect quietude, mild aperients, digitalis, &c., and certainly with great apparent advantage; at least the tumour did not increase in size, and the pulsation or jet of blood into the tumour became less distinct. He continued much in the same state for nearly five months, except that he gradually got thinner, probably from the confinement and low diet. The pulse never became so much affected at the wrist as might have been expected, from the manifest and extraordinary labouring of the heart, * which was both visible and audible, by exposing the chest, and making the patient stand in the erect position.

About six months from his admission into hospital, he complained of difficulty in swallowing, had considerable dyspnœa, and occasionally a little cough, accompanied with expectoration and night-sweats. About the same period, the modulation of his voice was observed to be altered. The pulsation in the tumour became less distinct, and its size remained stationary from a few days after its appearance; considerable emaciation soon followed, and it became obvious that he could not long survive. But the bursting of the tumour externally, appeared much less probable than a short time after its first appearance.

The dysphagia progressively increased, and at length became so troublesome, that the patient repeatedly expressed a wish that he might soon be relieved from all his sufferings. One evening when turning in bed, he felt the tumour burst internally, and immediately vomited about three pounds of blood. He had been so repeatedly warned that the complaint would prove fatal very soon after the tumour gave way, that he expressed himself as if he had only a short time to live. Soon after vomiting the blood, he said that he felt somewhat relieved; but was in a state of great exhaustion. On my arrival at the hospital, I found the pulse but indistinctly in both wrists, and

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* "The pulse at the wrist in aneurism of the arch of the aorta, is sometimes irregular; but often no irregularity can be felt in it."—*Baillie's Morbid Anatomy*, fifth edition, page 47.

the skin covered with a cold clammy perspiration. Considering the patient in a dying state, directions were given to keep him quiet, and occasionally to wet his lips with a little lemonade, or barley water. On the following morning, the patient was alive, though in a state of exhaustion which I had never before seen equalled. He had passed blood by stool to an amount which seemed equally incredible, and incompatible with a continuance of life—he could scarcely articulate, but, in answer to my inquiries, he said he felt a fainting sensation, accompanied with a great load at his stomach, and a pain in all his limbs—particularly in the joints.

It appeared manifest that the aneurism had burst into the alimentary canal; and any thing farther than a repetition of the injunctions about observing quiet, were not thought of. Upon my leaving the room, he made an effort to speak, and with some difficulty made the attendants understand, that he wished to know if any thing could be done to remove the load from his stomach. An ounce of sulphate of magnesia was ordered to be dissolved in seven ounces of water, to which a drachm of aromatic sulphuric acid was added, and directed to be given in small quantity at intervals, till the bowels were moved. About 3 pounds of blood was discharged by stool during the course of the day, and the stomach was a little relieved. He passed another pound of blood on the following day, * and to the surprise of all who were in attendance, gradually gained strength; and in three or four weeks appeared nearly as strong as previous to the rupture taking place. The external tumour remained flaccid, the cough, expectoration, and hectic fever nearly disappeared, and he could speak and swallow his food better than before. He continued in this state till about the beginning of April, when the tumour again began to fill, and, in short, all the former symptoms returned; and as the battalion was ordered to march to Dublin about the beginning of May, he was directed to be left in the hospital. This circumstance extremely disconcerted him: he had toiled through the hardships of the Peninsular war, and had an utter aversion to be separated from his regiment. Leave to accompany the corps to Dublin was refused for his own sake;—but on the morning the detachment was about to leave Cavan, he contrived to conceal himself in a waggon. When the circumstance was discovered, he was treated with kindness; and after

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* It was calculated that the patient lost in all from ten to fourteen pounds of blood; but as the quantity vomited was into a basin containing water, and as the remainder was mixed with the urine and feces, it was extremely difficult to ascertain the precise quantity.

travelling three days by the waggon, he arrived safely in Dublin. He was sent to the Royal Hospital, when he died in the course of a few days, apparently from cold caught while in the waggon. Owing to pressure of business at the Royal Hospital at the time, though a statement of the case had been sent with him, and an inspection of the body took place after death, the morbid appearances were not particularly noted. I was not apprised of his death till after the funeral.

The interest attached to this case arises principally from the gradual recovery of the patient after the rupture of the sac, which must be considered an unusual circumstance, and particularly after the loss of such an extraordinary quantity of blood. Whether the communication between the aneurismal sac and alimentary canal was accidentally closed by a coagulum pushed into the aperture, or by some other equally fortuitous circumstance, or whether a process for repairing the breach and continuing of life was commenced, must, in this case, remain a matter of entire conjecture; but from the gradual cessation of the discharge of blood by stool, the latter seems the most probable. From the length of time the patient lived after the rupture, it may be inferred that an ultimate recovery was within the reach of probability: indeed it was uncertain if the patient died from pneumonia, or from the pressure of the aneurism on the important parts contained in the thorax.

Kendal, 28*th April*, 1824.

X.

Medico-Topographical Sketches of Appleby, Westmoreland, and the Circumjacent Country. By William Bayers, Surgeon.

Appleby is situated not far from the centre of an extensive vale or strath, the length of which, from Kirkby Stephen to the junction of the Eden and Egmont, is nearly 20 miles; its breadth from Bampton to Dufton 12 or 13 miles. The principal valley is that of the Eden, in which Appleby is situated; but there are many subordinate valleys watered by streams tributary to the Eden, with beautiful irregular eminences between them. There are a few extensive thriving plantations, as the woods around Lowther Castle, at Dufton, Whinfell, &c.; but the numerous hedge-rows of trees, surrounding almost every field from some points of view, give this district the appearance of an immense forest. The Crossfell range of mountains on the east of this vale, is the highest land in this vicinity. The altitude of Crossfell mountain, as given in the late Trigonometrical Survey, is a few feet short of 3000 above the level of the sea. In point of elevation, the Mallustang range in the south, and the Shap range on the west, are considerably inferior to Crossfell. Haws water, the only lake in this district, is three miles long and half a mile broad. I know of no marshes in this neighbourhood.

Aspect of the Country, &c.—Appleby is the county town of Westmoreland; it sends two members to Parliament; it has a market on the Saturday, which is well supplied with grain. There are two large fairs in the neighbourhood; Appleby on second Wednesday of June, and Brough-hill on last of September and 1st of October. North latitude of Appleby 54° 32′, west longitude 2° 40′. The Earls of Thanet and Lonsdale are the principal landed proprietors in this district; but property is very much subdivided. A few gentlemen have estates from 1000*l.* to 3000*l.* a year; but the most numerous class of landholders are the yeomen, provincially called *statesmen*, who are possessed of property from 20*l.* to 400*l.* or 500*l.* a year.

Soil, Produce, &c.—The holm lands along the banks of the Eden, and some of the minor streams, are composed of a strong clayey loam; in some particular places a gravelly or sandy soil obtains; but the most prevalent soil of which the moorlands and eminences in a great measure consist, is a red or whitish clay, which, being imbedded upon a substratum of limestone or sandstone rock, long retains its moisture after rains.

A stranger, on entering this county, is much struck with the very minute fields he sees scattered up and down, many of them not exceeding half an acre. It would puzzle a mathematician to tell what figure many of these fields describe, so diversified are their shapes; but he finds, on inquiry, that most of these fields have separate owners. Wheat is much cultivated in the space between Eden and Lowther, on the east of the Eden, and the south side of Whinfell; on the northern extremity of the district, the soil being of a lighter gravelly consistence, barley and oats are more frequently met with. On these lighter soils, the harvest is often ten or twelve days earlier than on the clayey grounds adjacent. The turnip husbandry is a good deal attended to throughout the district. The long-horned breed of cattle is the most prevalent. The only indigenous breed of sheep is the black-faced; but the higher grounds being all common land, are often overstocked, which renders them frequently stunted in their growth. Every landed proprietor in the neighbourhood of these commons, according to the extent of his property, has a common right for so many sheep or cattle. Grouse abounds in the mountainous districts.

Population.—Appleby contains about 800 inhabitants. The population of this district lies very much in villages; several of the villages are nearly as large as Appleby. From what I know of some of its parts, I should estimate the whole population of this district at 13,000 or 14,000.

Inhabitants, Dwellings, &c.—The avocations of the mass of this people being almost exclusively agricultural, they are a strong healthy race,—their manners are simple, their habits temperate. Lowther Castle, the princely mansion of the Earl of Lonsdale, is the noblest structure this district can boast of. Appleby Castle, Earl of Thanet. Brougham Hall, H. Brougham, Esq. Acorn Bank, R. Hodgson Edmenson, Esq. Newbiggin Hall, —— Crackenthorpe, Esq. Hillbeck Hall, and a few more.

The dwelling of a statesman of 200*l.* or 300*l.*, consists of a parlour and back kitchen, a milk-house, and front kitchen, called by way of eminence the *house*, on the ground flour, and three or four bedrooms up stairs. The *house*, as it is called, is the usual sitting room and eating room of the whole family. One cannot help being struck at the very different manner servants are treated in England and Scotland, by persons of equal rank. A Scotch laird or farmer, with an income of 300*l.* a year, would think it a great degradation to sit and eat with his servants; the Scotchman perhaps treats his servants too superciliously, the Englishman with too much familiarity. The houses of the higher ranks, of course, are much superior to what I have described, those of the lower ranks much inferior; but the dwellings in this district, in general, are far from good. As one travels towards Carlisle, a progressive improvement can be perceived, in the appearance and comforts of the houses. The furniture is adapted to the houses, plain and substantial. Animal food is much more used in this district than formerly. Gooseberries and apples, in form of pyes and dumplins, are almost always seen at their tables; tea and coffee in the morning, even in the lower orders, has in a great measure supplanted crowdies and hasty pudding. Coal is almost the only fuel that is used.

Education.—One meets with a number of unlettered old people in this vale; but considerable improvements have taken place in this respect. Schools are established in almost every village, and more attention is now bestowed than formerly on the education of the young. There is a free grammar school at Appleby, which is well attended, and has produced a number of eminent scholars: it has exhibitions to Queen's College, Oxford. There is another free grammar school at Kirkby Stephen, and a third at Bampton.

Amusements, Customs, Morals, &c.—The natives of this vale are acute and intelligent in the subjects wherein they are conversant; but, generally speaking, they want that thirst after knowledge, that inquisitive and thinking disposition, which characterizes the lower orders of the Scotch. There are few readers among them, and few who have a taste for reading. No doubt many individual exceptions might be cited; and among the gentry and yeomen, I have observed those who would be an honour to any country.

The amusements are wrestling, leaping, playing at bowls, foot-ball, &c. The Sabbath is a great day for many of these exercises. They are often winked at, and even sometimes promoted, by the clergy. A late Vicar of Kirkland had a set of young men in his parish who often played the foot-ball all the way to church. When the congregation was dismissed, the Vicar was wont to give the ball the first kick, in order to set them a playing home again. This county is famous for wrestlers; it is a characteristic of one of the grammar schools, which shall be nameless, that it produces some of the best wrestlers in the north of England. Instances of crime, or of very depraved characters, are rare; but they allow themselves a latitude, especially on the Sabbath, which would be held opprobrious in some counties. Methodism, which is spreading fast in this district, whatever may be its defects, acts as a strong corrective of laxity of morals.

Mineralogy.—The mountain rocks in this district are chiefly a secondary formation. Limestone is found throughout its whole extent; the most prominent strata in the Crossfell range, consist of the Alpine limestone. Near Brough, the strata of limestone are very conspicuous. Greywacke is a constituent of the Mallustang range; while the Shap-fells, particularly those around Hawswater, incline to a primitive formation. Basalt and secondary porphyry are met with. Sandstone is not unfrequent. Detached blocks of granite are found in many parts of the country, but there is no regular stratification of granite, except a low range of rocks near Shap. Lead mines are wrought in the manors of Dufton and Hilton; the best metalliferous beds are almost exclusively found in limestone.

Meteorology.—Appleby having no great elevation above the level of the sea, not more I should think than 300 feet, and being begirt almost on every side with high mountains, the climate must be comparatively temperate. I do not know that any registers have been kept in this district, either of the barometer, thermometer, or pluviometer. The mean temperature of Kendal for five years, is rated at 46°; the temperature of Appleby, I expect must be considerably higher, both from its more sheltered situation, and less elevation above the level of the sea. This district is seldom visited with much snow, nor does the snow often lie long in the low grounds. With regard to humidity, I should think Appleby will hold an intermediate place between the western and eastern parts of the Island. The mean depth of rain at Keswick is 67.5 inches, at Kendal 60. inches, and on the eastern coast about 20. inches annually; if so, I should estimate the depth of rain at Appleby between 30 and 40 inches. The state of the winds in this climate is very variable; I have known a long series of boisterous weather, and I have known the reverse happen. The winter of 1819–20 was remarkably calm. During the greatest part of that winter there was scarcely a breath of wind.

A singular phenomenon connected with this subject, occurs in the Crossfell range, called the *helm wind.* During the time of its action, the summit of the whole range from Brough to Brampton, is covered with an immense white cloud, in form of a helmet, whence it is named. This cloud moves with great velocity, generally in a south-east direction. Exterior to this is a white space, apparently motionless; and at a still greater distance a dark cloud, parallel to the helm cloud, and of equal extent. From the helm cloud to this dark cloud are seen small dark stripes of cloud, constantly flying of. Near the summit of the mountain it is perfectly calm; lower down the wind rages with great violence, sweeping right down the mountain in strong irregular blasts: but never extends far into the country. During the continuance of the helm wind, a strong regular wind is kept up at Appleby and the vicinity; and on the other side of the mountain, the wind blows in an opposite direction from the helm wind. Many theories have been adduced in explanation of this anomaly, but none of them, I conceive, much to the purpose.

Pathology.—My observations in this department do not extend more than seven miles from Appleby, and are necessarily limited from the want of an infirmary or dispensary, where diseases are registered, and from the impossibility of obtaining information by necrotomy. Rheumatism, I observe, to be a frequent occurrence in this country. Many people lame from this cause may be seen in Appleby on a market day. I have seen all the fingers and toes dislocated by this disease; but it often produces anchylosis in the larger joints, as the hip, knee, shoulder, &c. The herpetic eruption, vulgarly called scurvy, is also very common. Scrofula, consumption and croup, are often met with. I have seen a number of cases of hydrocephalus. I remember a very interesting case of a young man, 18 years old. For three months previously to his applying for advice, he was affected with occasional acute pains in his head. When I first saw him, these pains had become more frequent and more continued. The outlines of the treatment consisted in copious and repeated depletion from the arm and temporal artery, cupping on the back of the neck, issues behind the neck, blisters on the head, blue pill with digitalis, friction with blue ointment on the region of the liver, &c. Notwithstanding all that could be done, the complaint went on increasing—a defect of vision came on, amounting eventually to complete blindness—next impaired hearing, proceeding gradually to total deafness—strabismus—great dilation of the pupil, &c. Convulsions at last closed the scene, but not before nine months from my first seeing him had elapsed. His complaint was judged by a neighbouring physician who saw him, a staff surgeon, and myself, to be hydrocephalus; but as no inspection of the body could be obtained, we had not an opportunity to ascertain the fact. Mr Abernethy remarks, in his Surgical Observations, that a person may have every symptom of hydrocephalus when necrotomy can detect no such thing. Epidemics very rarely occur; for five years I only met with five or six cases of typhus; a few cases of pertussis, no measles nor small-pox. It is often very difficult to induce them to have their children vaccinated, unless vaccination is generally practised in the neighbourhood.

Another most common complaint in this part of the country is bronchocele; there are few families where one or more of its members are not affected. Females are the principal sufferers; but I have also seen it in the male subject. When the tumour acquires a great size, respiration, and sometimes deglutition, are considerably obstructed by it. In recent cases, I have, in the course of five or six weeks, frequently succeeded in reducing the size of the tumour more than one half; but I seldom could induce them to persevere sufficiently long to obliterate it altogether. The means I used were friction, with simple, camphorated, or mercurial liniment—repeated blisters, burnt sponge, with or without calomel, carbonat of soda, &c. I have not had any cases of goitre since iodine obtained reputation for curing it. Of the many cases whence this disease is supposed to originate, I should think hard water, or water impregnated with carbonat of lime, as blameworthy as any. Such kind of water abounds in this district. Bishop Watson, in his Chemical Essays, says, that he never saw bronchocele in Westmoreland. I do not know what may be in the neighbourhood of Ambleside, where he resided, but this unseemly protuberance is certainly very prevalent in the Vale of Appleby.

In addition to those already enumerated, I might observe, that general and particular dropsies often occur. Asthma is sometimes met with; also enteritis and pneumonia, the last more frequent than the first. Dyspepsia is more common than one would expect, among people whose avocations are so much in the open air. Hepatic diseases are rather frequent. I know not whether the functions of the biliary system are more attended to now than formerly, but a large share is certainly attributed to its agency in the production of disease. I have also frequently had occasion to observe erysipelas, hysteria, leucorrhœa, menorrhagia, &c.

The next subject to which I would draw the attention of the profession is the pathology of miners, I mean those employed in lead mines. My experience in the diseases of this class of men has as yet been limited; but I have seen enough to afford sufficient cause for deploring the inefficacy of medicine in relieving many of their maladies. The gentlemen of the London Company, who lease most of the principal mines in Durham, Cumberland, and Westmoreland, with laudable industry have recently done much towards meliorating the state of the mines by the efficient modes of ventilation which they have devised. Still there are many places in the mines, to which, for a time at least, no ventilation can be applied. In the Durham and Westmoreland mines, carbonic acid is the most pernicious gas that is met with. Carburetted hydrogen is observed in Alston Moor; but I believe no atmosphere can be more noxious than the gaseous product arising from the explosion of gunpowder. This appears to be composed of carbonic acid, carbonic oxid, and azote—a compound evidently very deleterious; and with such an atmosphere the men are almost constantly surrounded when at work. It is very probable, too, that lead, in the state of impalpable powder, is frequently inhaled into the lungs.

A series of hard labour, and long working in bad air, make fearful inroads upon the constitutions of lead-miners. I have seen them at the age of thirty-five bearing all the marks of old age, and utterly unfit for labour; whereas, if the ventilation has been good, and they have not been overstrained with their work, they will continue workmen to the age of sixty. Yet few of them escape, in a greater or less degree, an affection resembling asthma, consisting in wheezing and difficult respiration, tightness across the chest, and expectoration of dark-coloured mucus. They are rather susceptible of catarrh, which greatly aggravates their other complaints. If a person in this state is attacked with pneumonia, he may be cut off with amazing rapidity. The practitioner has a nice course to steer. If bleeding is omitted or sparingly used, effusion into the lungs takes place: if depletion is carried too far, a degree of debility is induced, equally fatal to life. In such cases, I have been much perplexed. The paramount efficacy of bleeding, which has always been considered a sheet-anchor in inflammation, I was inclined to dispute. I have no doubt often witnessed the energy of judicious depletion; but I have also, especially among miners, met with very perplexing cases, where a tense vibrating pulse, acute pain in the breast or side, and difficult breathing, indicated the necessity of bleeding, and where, *a priori*, we would expect the detraction of blood to be very beneficial. I have carried it *pleno rivo ad deliquium;*—when the vital powers were recruited, I have again resorted to the lancet—as auxiliaries, I have applied blisters, and used other antiphlogistic means, but without effect. Such cases seldom terminate fatally; but the convalescence is slow and imperfect, and the constitution often permanently injured. Occurrences like these have a tendency to stagger our faith in the specific virtue of any medical agent, however high its character may be. In this state of mind I met with an account of the practice of Dr Pictet of Geneva, in inflammations of the chest, whose single remedy was very large doses of tartar emetic; and under this treatment he did not, for five successive years, lose one patient in pleurisy or peripneumony. Laennec also uses very large doses of tartar emetic in similar cases. I have latterly ventured to imitate this practice in pleurisy, &c., and apparently with good effect, yet still not without previous depletion. I have, in affections of the lungs, made trial of the vapour of a variety of substances, by means of an inhaler, but I cannot say with any marked effect. Patients very soon tire of this mode of exhibiting medicine, if it does not relieve them after two or three trials. Except the above affections of the respiratory organs, I am not aware that miners, from the nature of their employment, are incident to diseases differing much from those of other men; but when attacked with any serious complaint, this morbid state of the lungs renders them less able to struggle with it.

In your Journal for October last, a communication from Mr Braid, Leadhills, contains some etiological remarks respecting the inducing cause and frequency of pneumonia and hydrothorax. I have been at considerable pains to collect information, both from medical and other sources, regarding these diseases as occurring among the miners under the London Company; but the result of my own observations, and of the information I have obtained elsewhere, tends rather to militate against Mr B.'s conclusions. I have not found that pneumonia and hydrothorax are more frequent among miners than among any other class of men; nor have I been able to trace their origin to impure air, or working below ground. Perhaps the gaseous product of the mines, the routine of working, the habits of the workmen, and other local peculiarities of Leadhills, may be sufficient to account for the difference.

I have only heard of one examination of the body of a miner; he was broken-winded, as the miners' disease is provincially called, but not so much so as many of them. The immediate cause of his death was an obscurely marked complaint in the lumbar region, which was proved, by dissection, to be a very extensive lumbar abscess. No other morbid appearance presented itself in the abdomen. The inferior half of each lobe of the lungs was impervious to air, and could not be inflated from the trachea; the lungs in other respects were sound.

It may be laid down as an axiom, that mining, whether in lead or coal mines, has an obvious tendency to abridge the term of human life; but long working in levels proves most rapidly destructive of any. In this atmosphere, generally highly vitiated, the inhalation of the dust and small spiculæ struck off from the rocks where they work with picks, is extremely noxious; and some most suddenly fatal cases of consumption have occurred in persons who have been long exposed to the agency of such causes. Hence the London Company make it a rule that the men shall work by rotation in such places, and that none be employed longer than a quarter of a year in driving levels. *

Some curious instances of metastasis have been occasionally observed among miners; for example, symptoms of consumption will show themselves, and continue for some time with considerable severity; then a swelling, with succeeding ulceration, and caries of the bone, will occur in an extremity, as the ankle, when the phthisical symptoms entirely subside, and *vice versa*.

A person of veracity lately related to me the case of a miner, who, for a length of time, was much afflicted by dyspnœa, tightness across the chest, and severe cough; after expectorating a number of substances exactly resembling leeches (hydatids, I suppose), he became quite well, and continued so for many years.

It would rather seem, that, in modern times, we have several new diseases, and that diseases, which were formerly prevalent, are not now to be met with; and again, that diseases, which were formerly very rare, are now much more frequent. It is not long since croup was first noticed, and still later since it was discriminated from *laryngitis.* I have been informed by very intelligent old people in my native dale, that in their younger days, ague was very common, that they themselves were frequently attacked with it; and that palsies and consumptions were then very rare. The almost entire extinction of agues in the northern parts of the Island, has been attributed to the draining of wet and marshy lands. But in my native dale, there never have been any marshes, and the surface-draining made use of on the hills and pasture lands, can do nothing more than contract the trifling moisture of an extensive surface into narrower bounds. Whether the difference of food, difference of clothing, or difference of temperature, can have any effect in extirpating agues, I cannot pretend to say; that they are not now met with in these parts is certain, and I never heard any physical reason assigned which could satisfy my own mind.

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* For this, and various other useful information respecting miners, I am indebted to an intelligent agent in the employ of the London Lead Company.

State of Surgery, Quackery, &c.—The number of practitioners in this district is fourteen; four at Appleby, one at Temple-Sowerby, two at Shap, one at Orton, four at Kirkby Stephen, two at Brough. In this number there is a considerable proportion of respectable surgeons. There are still a few midwives in this district, but their number is decreasing, and their practice diminishing.

This country is very prolific in quacks and charlatans of various descriptions. The bone-setter general for Cumberland and Westmoreland is pupil and successor of the notable Ben Taylor, whose fame, I doubt not, has reached the metropolis of Scotland. No accident can befal the bones or ligaments, but D. must be consulted. Almost every village, too, has its bone-setter. To this is generally annexed the craft of farrier and cowleech. Some of them also make great pretensions to the knowledge of the nature and cure of disease. One of them told me, if I would only visit him frequently, and abide by his directions, he would make me the first doctor in Cumberland or Westmoreland. I dare say the old gentleman was rather mortified that I did not accept of his invitation. I have no doubt that their extensive practice in the reduction of fractures and dislocations, often enables them to foil the regular surgeon who has no such opportunities.

A highly respectable lady had the first joint of the middle finger dislocated, or otherwise injured. She had consulted many surgeons respecting it, and, among others, some eminent surgeons of Edinburgh, but derived no benefit. She happened to be on a visit at Appleby; D. was making his rounds, and was consulted. How he managed his manipulations I know not; but in course of two or three days she could write a letter, which she had not been able to do for months before. The lady in whose house she was, observed to me, that it was a satire on the whole College of Surgeons.

All my osteological practice for five years amounted to no more than the adjustment of four or five fractures; one of them was a fracture of the middle of the *os humeri*, accompanied with a fracture of the *occiput* nearly three inches in diameter, in a boy 15 years old. He met with the accident by a fall of sixty feet into a coal pit;—he recovered perfectly in four weeks, without exhibiting any symptoms of compressed brain.

There is a set of medical magicians in this district whom I never saw nor heard of any where else. By visiting the patient and muttering over a certain incantation, they pretend to stop hemorrhage, or cure scalds and burns; but beyond these their sway does not extend. I have not been able to learn what the incantation is, nor in what language it is expressed; but their power to suppress the one or cure the other *instanter*, is not at all questioned by the mass of the population. One of these conjurors was completely baffled in a case of profuse, periodical epistaxis, in a young man aged 26; the bleeding recurred every two or three weeks to the extent of 1 or 2 pounds at a time. He afterwards went to Penrith, and expired under an alarming attack of hemorrhage.

There is another sort of quacks who inherit from their ancestors, perhaps as far back as the days of Thomas the Rhymer, a secret recipe for curing some particular disease—for instance worms. I know a female practitioner of this stamp, who was famous for worm plasters: this plaster was applied over the stomach, and was infallible in its effects,—at least it was considered so.

Biographical Notices.—Any records of this district I have access to, are rather barren of intelligence respecting medical practitioners or authors.

A Dr Barwick was a native of this vale,—he married a neice of Archbishop Laud, and settled in London, where he attained to much eminence in his profession.

Thomas Gibson, M. D. physician-general to the army, and author of a System of Anatomy, was also born in this district. His second wife was a daughter of Richard Cromwell the Protector.

Dr Pearson, an eccentric individual, though not a native of Westmoreland, was universally known in this district, and while he lived, was almost the only physician employed. He was born at Lazonby, near Kirkoswald in Cumberland, sixteen miles from Appleby. He practised for some years as physician in Newcastle-on-Tyne. He became extremely dissipated in his habits, and returned to Lazonby, where he had considerable property, and there spent the remainder of his life. He was held in great estimation for his professional abilities by the country people, both in Cumberland and Westmoreland; he was a very powerful man, more than six feet high, and proportionally strong and athletic. The anecdotes respecting him are innumerable, and, if collected and arranged, would form a tolerable article for the Eccentric Mirror. His manners were coarse and abrupt, yet manifested much shrewdness and information, though generally mistified by inebriety. He could adapt himself to any company, from the peer down to the potter or tinker, and never was more in his element than when carousing amid a squad of tinkers or such like. When sent for to Appleby or the neighbourhood, he would often be one or two days in accomplishing the journey, by calling at every public house by the road. In a protracted drinking-bout at Penrith, he drank himself delirious, and fancied he saw spectres hovering round ready to carry him off. In this state he attempted suicide—the wound was not mortal, but it produced a complete and permanent change in his sentiments and conduct. From a life of extreme intemperance, he betook himself to habits sober, regular, religious. I asked Mr Law of Penrith, an intelligent surgeon who attended him, what sort of a man Pearson was: he said he was a person of considerable and diversified talent, but a careless practitioner. Subsequently to this, Mr Law happened to call upon him. Pearson was busy reading;—he said he had now confined his researches to one book (pointing to a bible lying on the table), but that book was the best. In a corner of the room lay a number of volumes piled up; these, Pearson said, were Bolingbroke, Hume, Voltaire, Paine, &c.; books, in the perusal of which, he was wont to take much pleasure;—since his change of sentiments, he had collected all the infidel volumes he could lay his hands upon, with the intention of burning them, but he had now altered his purpose, and meant to write against them, to counteract, if possible, the mischievous tendency of those sceptical principles he had been at so much pains to disseminate. He was killed by a fall from his horse eight or nine years ago. I have not heard that he ever published any thing on the subject of medicine. He wrote a dissertation explanatory of the phenomenon of the helm-winds on Crossfell, which I have not seen.

Dean Launcelot Addison, the father of the celebrated Joseph Addison, the father of Hogarth the painter, and Langhorne the poet, were natives of this vale.

29th January, 1824.

PART II.

CRITICAL ANALYSIS.

I.

De l'Influence des Agens Physiques sur la Vie, &c. On the Influence of Physical Agents on Life. By W. F. EDWARDS, M. D. Member of the Royal Academy of Medicine, of the Philomathic Society of Paris, and of the Medical Society of Dublin. Paris, Crochard. 1824.

WE take this early opportunity of bringing under the notice of our readers the most valuable and interesting work on Physiology that has appeared for many years. It is the work of an Englishman, educated and naturalized in France, who at an early period devoted himself to the pursuit of Experimental Physiology, and has been for some time preeminently distinguished among the crowd of well-known aspirants who have successfully cultivated that science at Paris.

A considerable proportion of the work now announced has been already before the public in the form of detached Essays, which, as the author advanced in his researches, were read from time to time before the Institute, and published in the *Annales de Chimie et de Physique*, and in Magendie's Journal. These Essays have twice gained the annual prize of the Institute for Experimental Physiology, namely, in 1819 and 1821. They are not much known, however, in this country: Even our periodical works have taken little notice of them; and the profession in general are wholly unacquainted with them. We are conscious, therefore, that we shall discharge a grateful task both to the author and to our readers, in laying before them a full and connected view of his whole discoveries. This we propose to do, by giving a simple analysis of the book, without comments or criticism.

In laying down the plan of the work, Dr Edwards announces, that his attention is to be confined to those physical agents which constantly surround us, and are continually exerting their influence on our bodies—namely, the air in its several relations of quantity, motion, and density—water in its fluid form and in its state of vapour—heat in its modifications of degree and duration—light—and electricity. Each of these influencing powers he has examined, first, as it affects animals of the family of the *Batrachi*, or frogs; secondly, other vertebrated and cold-blooded animals; thirdly, warm-blooded animals; and, lastly, man and vertebrated animals generally. Under the last head he exhibits in one connected view all the facts determined in the previous part of his work, as well as those on the same subject which have been ascertained by former physiologists. The work is then concluded by an Appendix on Electricity, for which the author is indebted to the pen of MM. Prévost and Dumas.

He begins with the cold-blooded animals of the family of the Batrachi, because they present the widest field for observation, both with respect to the duration of the phenomena, and the facility of multiplying the requisite experiments.

I. The first object of the author's experiments was to ascertain the precise influence of the air on the life of animals of the frog-tribe; and with this view he has examined with great care the effects which result from the deprivation of that element. The first step in this inquiry was evidently to determine, whether the various media, in which the animals might be placed during the experiments, had any other influence on life besides what flowed from their influence on respiration; that is, whether the media had any direct power over the muscular and nervous systems. This object was easily attainable, in consequence of the facility they possess of living after the circulation and respiration have been arrested. Accordingly, he cut out the hearts of several salamanders, frogs and toads, left a few of each in the atmosphere, and placed as many in water deprived of its air by boiling. The salamanders lived seven or eight hours in the water, and from twenty-four to twenty-nine hours in air; and a similar difference, though not so marked, was also observed in the experiments with the toad and the frog. He also found, that if a frog was immersed in water till no sign of life was left, it speedily revived on exposure to the air. Consequently the air affects these animals by directly impressing the nervous and muscular systems, independently of any power it may likewise possess through the medium of respiration.

Having ascertained this interesting fact, Dr Edwards next proceeds to inquire what power the deoxidized or venous blood has in sustaining life. This he ascertained by immersing frogs in water deprived of its air, some of them being entire, while, in the remainder, the circulation had been previously arrested by cutting out the heart. The former always lived longest, sometimes even 20 hours longer than the latter. With the toad and the salamander the phenomena were precisely analogous; and it is, therefore, clear, that in this order of animals the circulation has the power of supporting life for a long period, even when it transmits only a venous blood. The duration of life after the suppression of the breathing would naturally be expected to be still greater when the animal is left in the air instead of being immersed in water. For we have just seen that water as a medium is less favourable than air to the vitality of the nervous and muscular systems. Accordingly, when the access of air to the lungs was wholly prevented by a tight ligature round the neck, he found that the frog lived from one to five days in the air, while it died in ten or twelve hours under water. The difference was even more remarkable in the case of the salamander: one of them lived eleven days in the air. This great prolongation of their life, however, is not altogether owing to the direct action of the air on the nervous system, but also, and in a greater degree, to the action of that agent on the blood through the skin. For Spallanzani long ago remarked, that carbonic acid is formed in air in which frogs had been kept without breathing; and Dr Edwards has confirmed the fact by several very careful observations.

All Dr Edwards's experiments, however, tend to show that respiration is necessary to the existence of these animals, and that although they can live some time without it, yet when the access of air to the lungs is completely prevented, they always perish sooner or later.

It therefore became an object of interest to discover what merit is due to the often-told stories of toads living for periods of an extraordinary and almost inappreciable length sealed up, as it were, in the heart of trees, and stones, and apparently deprived altogether of air. A kind of authenticity has been stamped on these stories by the experiments of Hérissant, made under the eye of the French Academy in 1777; for he found that of three toads closely immured in boxes with plaster of Paris;—two were alive when the boxes were examined after an interval of eighteen months. Dr Edwards has shown that the Academy, however careful they seemed to be in removing every obvious source of fallacy, nevertheless admitted one of most essential importance. He first repeated the experiments of Hérissant, and obtained analogous results. The apparently asphyxiated animals lived a great deal longer than when they were asphyxiated in the air, as already described. Nay, however paradoxical it may appear at first sight, they lived longer so asphyxiated than when they breathed freely in open air. But when thus immured, the air is not prevented from reaching them. In fact, Dr Edwards found, that if a tube closed at one end with plaster was filled with mercury and reversed in a mercurial trough, the air penetrated the plaster with great rapidity; and consequently that when the boxes containing the animals were plunged under water or mercury, they lived very little longer than those which had not been previously immured in the plaster. The paradox noticed above is also easily accounted for. When these animals are left uninjured in the open air, they soon perish through the loss they sustain by transpiration and consequent desiccation; but when shut up in plaster, while sufficient air is admitted to oxidize the blood, transpiration is rendered exceedingly slow.

The progress of asphyxia in the frog-tribe is most singularly affected by various external circumstances,—namely, temperature, season, and the presence of air in the water in which they are immersed. The operation of each of these modifying circumstances has been investigated by our author with consummate ability; and the greater part of this investigation is entirely new. Spallanzani and others, indeed, had remarked that frogs live longer under water in winter than in summer, but they never endeavoured to examine the fact particularly, or to trace it to its causes.

1. *Temperature.*—When frogs during summer were placed under water varying in temperature from 59° to 65°, they lived from one hour to two hours and a half; but if, at the same season, the temperature of the water was reduced to 50°, they lived betwixt five hours three quarters, and six hours one quarter, or fully twice as long; and when the water was brought down to the freezing point, they survived from 6⅓ to 8¼ hours, or thrice as long. On the other hand, if the temperature was raised to 72°, they survived only from thirty-five to seventy minutes; at the temperature of 90°, they lived only from twelve to thirty-two minutes; and at 107° they always expired within two minutes, and some in a few seconds only. Results precisely similar were obtained with toads. In performing these experiments, the utmost care was taken that the concomitant circumstances should be alike in all, and that the temperature only should vary.

2. It likewise appears that the gradual change of the seasons effects some alteration in the constitution of animals of this tribe,

in consequence of which, their capability of living without respiration is very much modified. In the months of July and September, when the temperature was 65°, we have seen that they died under water in two hours and a half at the farthest. But in November they lived under water raised to the same temperature between $2^h\ 5'$, and $5^h\ 35'$. The author runs rapidly over the chief circumstances which might be supposed to produce this phenomenon, and shows, in the end, that it can be attributed only to the influence of previous temperature, the average of which, for a month before the first experiment, was 60°, and for a month before the second, 46°. A similar prolongation of life also takes place at lower temperatures. Thus when the water was at 50°, they lived under it from $5^h\ 10'$ to $11^h\ 40'$ in the month of November, from 20 to 24 hours in December; and at the temperature of 32°, in the latter month, some of them survived two days and a half. Life, however, is not prolonged at high temperatures, by the influence of the cold seasons. In water, at the temperature of 107°, they always perish within two minutes.

3. But the circumstance which affects most the life of the frog-tribe, when confined under water, is the presence of air dissolved in it. Passing over the observations, experiments and reasoning, quoted from previous authors, as too vague for the purposes of physiology, we shall proceed at once to notice the general facts which Dr Edward's himself observed. Even when the quantity is limited, he found that a frog lives in the winter time nearly twice as long in aërated water, as in that which has been deprived of air by boiling. But if drawn off daily and replaced by fresh water, care being taken to keep the animal always immersed, it lives many months; one survived more than two months and a half, and even then died only in consequence of the renewal of the water having been one day accidentally forgotten. This extraordinary prolongation of its life can be owing to nothing else than the supply of air from that contained in the water; for if the water is replaced by portions previously deprived of air, the animal perishes in a few hours; and, besides, as long as aërated water is supplied in sufficient abundance, the vessels of the web between the toes may be seen to convey arterial blood. It appears, therefore, that the frog is truly amphibious, although it does not possess the combination of organs with which other amphibious animals are provided; and consequently that it may pass the whole or greater part of the winter under water, as some naturalists have alleged. But it is not torpid at that time, as has been generally supposed. Its motions indeed are sluggish; but still it does move about, and is sensible.

It may be objected to the results thus obtained by Dr Edwards, that according to Spallanzani, the frog dies sooner in a stream than in stagnant water. But our author repeated that experiment, and found the fact to be quite the reverse. This property, however, is not possessed at all temperatures and at all seasons. In the month of March, when the temperature of the water did not exceed 50°, a frog died in twenty days, though the water was renewed daily; and in April, at the temperature of 56°, another perished in a single day. If they were placed in a stream of water, however, they lived much longer at these temperatures than in a limited quantity daily renewed; yet almost all of them died within a month. The difference of course depends on the regular supply of a more perfectly aërated water.

Through what organ therefore is it enabled to abstract the air from the water? It cannot be the lungs; for the movements of deglutition, which always accompany the breathing of the frog, cease the moment it is plunged under water. It can only be the skin; and that this is really the organ of respiration in such circumstances, Dr Edwards gives ample and direct proof in a future part of the work.

Having thus ascertained the influence of aërated water on the life of the frog-tribe, the next object of our author's researches was to determine the vivifying influence of atmospherical air itself, acting on the skin and on the lungs. The power which the atmosphere possesses of maintaining their life, by acting on the skin alone, is nearly the same as that possessed by aërated water frequently renewed. This was put to the test of experiment in several ways; first, by keeping the mouth open by a gag, in which state the frog can breath but very little; secondly, by applying a tight ligature behind the head; and, lastly, by cutting out the lungs. (In making these experiments, care must be taken to keep up a sufficient supply of moisture, otherwise death speedily ensues from desiccation.) When altogether deprived of the lungs, which Dr Edwards considers the best mode of suppressing respiration, it lives from thirty-three to forty days. This period is considerably shorter than the duration of their life in aërated water at low temperatures; a circumstance which may be safely attributed to the effect of the operation; for, when animals so mutilated were placed in water, they too died considerably sooner than those which were uninjured. At all events, the influence of cutaneous respiration in supporting life is evidently very great. It is not so easy to determine the influence of pulmonary respiration alone. If the function of the skin be destroyed by stripping it off, the frog dies in a few hours; or if its body be immersed in oil, and its mouth held in the air, it perishes during the first day: But neither of these modes of experimenting is free from objection. A less objectionable mode was by immersing the body of the animal in water of such a temperature that it could not live by the absorption of the dissolved air, and keeping its head always in the atmosphere. When so situated, the frog survived three months and a half; but other animals of the same natural family, such as the *rainette*; and one variety of the toad (*crapaud accoucheur*), died in three or four days. It would appear, therefore, that some, if not all of the *batrachians*, are incapable of living long by the pulmonary respiration alone.

Hitherto we have been considering the influence of air, water and heat, on the life of the frog tribe, merely as they affect the respiration. We shall now proceed with our author to relate their effects on Transpiration and Absorption. It was natural to expect, that the loss sustained by transpiration in the atmosphere would, *cæteris paribus*, decrease with the time. This was determined experimentally by placing various species of the *batrachians* in calm air of uniform temperature, and weighing them at regular intervals. These intervals, however, must be pretty long; for the transpiration is so powerfully influenced by trivial causes, that if the observations were taken at intervals considerably shorter than three hours, the loss was found to be very irregular, being sometimes greater, sometimes less than during the previous interval. But if the weight was taken every third hour, it became evident, that the loss diminished progressively with the time. The reason is obvious: Each successive diminution leaves less and less fluid for supplying the subsequent transpiration.—The effect of the state of the air as to repose, in modifying the loss by transpiration, is very great. A frog placed at an open window lost, according to the force of the wind, two, three, or four times as much as another left in the same apartment at a shut window. The influence of the hygrometric state of the air is likewise very remarkable. In calm air saturated with moisture transpiration still takes place; a greater loss is sustained than can be accounted for by the chemical changes produced by the breathing. But when the air is kept as dry as possible by quicklime, the loss is five or even ten times as great. The influence of temperature is also obvious, though not so great as might have been anticipated. At 68° it is twice as great as at 32°, and at 104° it is seven times as great. At high temperatures, too, the loss is much more uniform, being nearly equal in equal times till the animal is near the point of death.

The phenomena which take place in water are of course very different. If a frog, which has been reduced by transpiration in the air, be put in water, it recovers its weight in a much shorter time than it lost it; and the increase diminishes as it approaches the point of saturation. When it has arrived at this point, it does not remain stationary. It alternately loses and gains weight; but the loss eventually predominates. The occasional loss is owing to the transpiration of animal matter, the occasional gain to the absorption of water, and the ultimate decrease to a diminution in the power of absorption, from want of nourishment. This transpiration of animal matter must be distinguished from general transpiration, such as takes place in the air. The latter consists partly of the former, or *transudation*, as Dr Edwards distinguishes it, and partly of transpiration by *evaporation*. Evaporation is most abundant in the air, but cannot take place in water; Transudation is a vital, evaporation a physical process. The transpiration in water, or transudation, is very much increased at high temperatures, so as to exceed considerably the absorption; and the reverse takes place at low temperatures. It is probable that *transudation* in the air is also much augmented by increase of temperature. The effect of the dryness and motion of the air, on the other hand, appears to be directed on *evaporation* chiefly.

II. We have now concluded our analysis of Dr Edwards's Experiments on the Influence of Physical Agents on the Life of the Frog-tribe. The second part of the work embraces an account of their influence on other cold-blooded vertebrated animals, such as fishes, lizards, snakes and tortoises. He has prefixed to this department of his subject a short and interesting inquiry into the influence of these agents on the life of the tadpole, and on its transmutation into the perfect frog. We need hardly remind our readers, that the tadpole possesses the double pulmonary system of true amphibious animals. Hence Dr Edwards found, that, even when the temperature of the water exceeded 50°, at which degree frogs soon die if prevented from breathing, these animals continued long vigorous. He has kept them many days under water at 77°. It is well known that their transmutation into the adult state is very much affected by nutrition and temperature; an imperfect supply of nourishment and cold retard it considerably. Our author has added the singular fact, that it is likewise much retarded by preventing them from having access to the air. Twelve were sunk in the Seine in a tin box perforated with holes, and others were kept in water from the river, which was exposed to the air, and changed frequently. All of them had nearly attained the size at which their transformation takes place. In a few days the latter underwent the expected change; but of those which were sunk in the Seine, ten continued in the tadpole state long afterwards, and had grown twice or even thrice as large; and the other two underwent the change much later than those which had free access to the air.

The author next proceeds to examine the relation which subsists between the frog-tribe and fishes, with respect to the influence of physical agents on their life. When fishes are placed in water deprived of its air, temperature has precisely the same effect on them as on frogs. At the freezing point they live longest; at 104° they die within two minutes. In a limited quantity of aërated water excluded from the contact of the atmosphere, the effect of temperature is the same. If the water is exposed to the atmosphere, the result is nearly the same as in the case of the frog. At the temperature of 68° the bleak *(ablette—cyprinus alburnus)* dies in a few hours; but at 50° or 53° it survives till the water spoils, and will live indefinitely, if the water be renewed occasionally. So much with respect to their life in water.—Fishes, it is well known, are not in all circumstances capable of living independently of respiration in the air; it has been shown by former physiologists, that they sometimes rise to the surface to breathe in the air, and that they die if confined altogether under water. Dr Edwards has extended considerably our knowledge respecting their respiration in the air. He shows that their gills can exercise, to a certain degree, the same function as the lungs of terrestrial animals. Thus the fishes which do not move their gills in the air, die much sooner than those that do; and their existence is rendered as long by keeping their gills open. Farther, fishes die in the air not so much perhaps from a change in their mode of breathing, as by exhaustion from the loss they sustain by transpiration. The gudgeon (*cyprinus godio*) and the pollard (*C. jeses*) lose about a fourteenth part of their weight by transpiration before they die; which Dr Edwards seems to consider to be alone an adequate cause of death; and, in consequence, when moisture is supplied sufficiently to them, their existence in the air may be extended to 9½ hours. He appears disposed to conclude, partly from reasoning, and partly from experiment, that this term might be considerably prolonged, if a mode could be devised for maintaining uniform imbibition throughout the body, while the gills act in the air. His reasoning does not admit of abridgment, and therefore we cannot admit it here, more especially as it does not appear quite satisfactory.

The author then concludes the second great department of his subject, by describing cursorily the general results of his experiments on lizards, serpents and tortoises. These results are quite analogous with those obtained in his experiments on the frog-tribe. The few differences which may be observed depend either on the greater extent of their lungs, or on the greater thickness of their cuticle. On the former account serpents and tortoises can live in the air by their pulmonary respiration alone; on the latter they are incapable of living long under water; while the lizards, on account of their nearer approach to the frog-tribe in the size of their lungs, and the texture of their skin, resemble them likewise much more closely in their mode of life.

III. The third grand division treats of the Influence of Physical Agents on Warm-blooded Animals, and includes seven chapters.

The three first chapters treat of the power of producing heat possessed by young animals, by adult hybernating animals, and by other adult warm-blooded animals.

1. It has been a common idea, he remarks, that young animals have a higher temperature than adults. But the notion is not founded on actual observation, and he has never been able to observe any fact in conformity with it. The temperature of puppies is always very nearly the same with that of the mother, or from one to four degrees less. If young puppies, kittens or rabbits, are removed from their mother, and left in air between 50° and 68°, their temperature sinks rapidly, and in three or four hours it is only a few degrees above that of the surrounding atmosphere. In this respect, then, they resemble the cold-blooded animals. The cooling is quite unconnected with deficient covering; it takes place equally with the puppy and kitten, which are born well clothed, as with the young rabbit, which is born almost naked; and it is not prevented by artificial covering. It arises, therefore, from a deficient production of heat. It is observed most remarkably soon after birth; as they increase in age and strength, they cool more and more slowly, and in 15 days they preserve their temperature like adults. All mammiferous animals are not so circumstanced. Thus, the youngest guinea-pig preserves its temperature as well as those which are grown up. The deficient power of generating heat is probably connected with the same cause which renders some of the other functions imperfect at birth. Dr Edwards has observed it always and only in those born blind. A similar deficiency exists in the young of many birds. Spar-

rows eight days old, if kept apart, cool very rapidly to a temperature a few degrees above that of the air. Hawks are similarly circumstanced, and so are the young of all birds which are born naked and unable to run about and feed. Those born with down, and which can run about and feed themselves, can maintain their temperature at birth. The others acquire this property in the course of the third week. But after they are able to sustain their temperature in mild air, they will still show a deficient power of producing heat, if the air is rendered very cold. Thus, a young magpie whose temperature was steady in mild air, having been kept in air cooled artificially to 37°, lost 25 degrees of heat in twenty minutes, while an old bird lost only six in an hour. Similar results were obtained with the blackbird, loriot, jay, and starling. In all these experiments care was taken to abstract the effect of differences in bulk or covering. They prove that the young of many birds and mammiferous animals are closely allied in their faculty of generating heat to animals of cold blood.

2. But some of the mammalia retain this affinity to the cold-blooded animals even in the adult state. For it is to a deficiency in the power of producing animal heat that must be ascribed the torpid state of hybernating animals. Naturalists have at different times imagined that their torpor in the winter season arose from a change in their organization, or from the want of proper nourishmnet. But various experiments have more lately shown that it is really connected with an incapability of preserving their temperature in cold air. It was observed, in the first instance, that during hybernation the heat of their bodies was only about 50°, while in the spring and summer it was 95° or 99°. Afterwards M. de Saissy found, that this difference in animal temperature was effected by gradual changes, and that the descent was considerable before hybernation commenced. Thus, a marmot lost 9°.5 betwixt the beginning of August and end of September, and 7°.25 between that and the end of November; that a *lerot* lost between the same periods 10° and 18° ; and a hedgehog 3°.5 and 38°.5. Finally, the same observer found, that he could render a hybernating animal torpid even in summer by exposing it to great cold. Having exposed a marmot to an artificial cold of 14°, its temperature fell from 95° to 41° in eleven hours, and it became completely torpid. On bringing it again into the warm air, it soon recovered its activity. Dr Edwards has repeated this experiment on the bat in the month of April. When kept in an artificial temperature of 34°, its heat fell in one hour from 93° to 57°.25.

3. Such being the fact with respect to hybernating animals,

it becomes an object of much interest to determine whether the other warm-blooded animals do not possess a greater power of generating heat in the cold than in the warm season. For it is now generally acknowledged that their temperature is almost, or even altogether uniform, or, at all events, that no greater difference is observed at different seasons, than has been likewise observed in different individuals of one species in the same season. One would naturally expect, therefore, to find the faculty of generating heat greater in winter, otherwise how does the body withstand the increase of cold in the atmosphere? Dr Edwards has settled the point with respect to birds by direct experiment; and analogy, as we shall afterwards find, will lead us to infer, that the mammalia are similarly constituted. In February, five sparrows were placed in a vessel cooled by a mixture of ice and salt—proper means being at the same time used to enable them to breathe freely. The mean diminution of their temperature in one hour was three-fourths of a degree, and no greater decrease was effected at the end of the third hour. In July this experiment was repeated on four sparrows. In one hour the mean decrease was 6°.5, and in three hours 11°. In the month of August it was 3° in one hour, 8.75 in three hours. It is hence evident, that a continuance of warm weather impairs their power of producing heat.

The three subsequent chapters of this division are occupied with an inquiry into the respiration of warm-blooded animals.

4. All warm-blooded animals require the free access of atmospherical air to their lungs; if completely excluded, few of them survive five minutes; this term will be sufficient to kill even those which live in the water, and are accustomed to plunge under it. But it is a singular fact, first observed by Buffon, that the young of some warm-blooded animals can support the privation of air for a much greater length of time. He found that new-born puppies may be kept half an hour under warm milk without suffering any injury. Legallois made a similar observation on young rabbits; having found that the limit of their existence in a state of asphyxia was about half an hour. This faculty of resisting the privation of air diminishes rapidly with their progress in age. On the second day, indeed, or sometimes even on the third, the difference is not very obvious. But, on the fifth day, they survive only sixteen minutes; on the tenth only five minutes and a half, and on the fifteenth they die as soon as adults. The facts now mentioned are very remarkable, as they concur precisely with those formerly noticed with regard to their power of producing heat. At birth, and for two or three days afterwards, the power of producing heat

is feeblest; on the fifth day it is decidedly greater; and in fourteen days it is almost as perfect as in adults. But we have seen that the power of producing heat is in some animals perfect at birth. If, therefore, the coincidence just noticed is not purely accidental, there ought to be in the young of these animals a corresponding incapability of enduring the privation of air; and Dr Edwards has shown that the correspondence is exact. A young guinea-pig cannot support asphyxia above three or four minutes longer than an adult of the same species. The same agreement may be observed in birds. A young sparrow, which, in air at 61°, lost, in half an hour, 29 degrees of temperature, lived eight minutes under water; while another, which lost only 16.°25 died in four minutes; and the adult sparrow survives but one minute and a few seconds.

Temperature has a considerable influence on the duration of asphyxia. In water at 32°, kittens one or two days old live only four minutes and a half; at 50°, they live ten minutes and a third; at 68°, thirty-eight minutes and three quarters; at 86° the period begins to shorten again; and at 104° they survive only ten minutes and a half. Analogous phenomena may be observed in adults, but of course the differences are very trifling.

5. The next object of the author's attention is to discover whether any relation subsists also betwixt the power of producing heat, and the consumption of air by breathing. This chapter is begun with a brief summary of the chief facts previously ascertained concerning the changes produced by respiration in the air. The only one of these facts which it is necessary for us to mention, is, that all warm-blooded animals, when confined in a limited quantity of air, consume it to the same extent before they die; and that if others are placed in air so altered, they perish as soon as when they are deprived of air altogether. Dr Edwards has found, that with respect to the duration of life in a limited quantity of air, considerable differences exist among animals even of the same species, age and size; but all reduce the air to the same state of impurity before they die; and consequently such differences probably depend on constitutional peculiarities, which modify the rapidity with which the air is consumed. These peculiarities are very striking, if we take, as subjects of experiment, young and adult animals of those species which are born imperfect. Were we to judge, says he, of their power of consuming air by the rapidity of their breathing or circulation, or the activity of nutrition, we should assume at once that young animals effect its consumption most rapidly. But the reverse is the fact. In a given quantity of air, in which adult sparrows lived at an aver-

age one hour and thirty-one minutes, young sparrows eight days old lived fourteen hours and fifty minutes;—a difference which is much greater than can be accounted for by the difference in their size, the former being only twice as big as the latter. If the experiment be made on the young bird when it has just begun to feed itself, at which time it is as big as the adult bird, the difference is much less, though still perceptible; it lives at an average two hours thirty-nine minutes. We see again, therefore, a striking coincidence betwixt the power of producing heat, and another important phenomenon in the life of animals: Sparrows eight days old cannot maintain their temperature, and they just begin to be capable of maintaining it, when they begin to feed themselves. Similar facts have been established by Dr Edwards with respect to the mammalia. Puppies one or two days old were kept in a limited quantity of air for four hours and fifty-nine minutes. Guinea-pigs fifteen days old were then kept in the same quantity of air till it was similarly consumed; and the time required to effect this was only one hour and forty-two minutes, or one-third of the time required by the puppies. Yet the latter were even larger than the former. We need hardly remind our readers how singularly this fact accords with their relative power of producing heat. For a description of the apparatus employed in these experiments, and the collateral circumstances to be attended to, we must refer to the work itself.

6. It only remained for the author to inquire, whether the same coincidence may be observed relatively to the seasons of the year. We have seen that adult warm-blooded animals produce less heat in summer than winter. Do they also consume more air in the former than in the latter season? In the month of February he shut up six yellow-hammers (*bruans*) in a limited quantity of air, heated artificially to 68°, and found that the mean duration of their life was one hour and 2⅔ minutes; while in the months of August and September, the mean of thirteen experiments, made under similar circumstances, was one hour 22 minutes. He likewise obtained analogous results with the green-finch (*verdier*); the mean term of their existence in January being one hour 9⅓ minutes, and in August one hour 33 minutes. Lest any one might object to this mode of making the experiment, that the mere duration of life is not a sure criterion of the consumption of the air, our author likewise observed at what period the birds showed the first signs of impeded respiration. In winter this happened in 52½ minutes, in August in 69 minutes. These experiments were of course conducted with due attention to uniformity as to the temperature of the air in which the birds were included, and the pressure of

the atmosphere. For Legallois found, by experiment, that the consumption of the air by breathing is retarded by augmenting its volume; an effect which every one knows is produced by heating it, or by lessening the pressure of the atmosphere on it.

7. The last chapter of the present section is appropriated to the Transpiration of warm-blooded animals. In them, as in animals of cold blood, the loss sustained by transpiration appears very irregular, if the observations be taken at short intervals; but if they be made at intervals of two or three hours, it is found to diminish progressively with the time. Dr Edwards ascertained this by experiments with guinea-pigs, mice and sparrows. The state of the air as to humidity has an important influence on the transpiration of the warm-blooded, as well as the cold-blooded animals. Thus, if in air saturated with moisture, sparrows lose at an average 2.62 grains in six hours, they will lose, in air of the same temperature, and kept as dry as possible by quicklime, 16.06 grains, or six times as much; and the difference would probably be greater, if perfect dryness of the air could be insured throughout the experiment, as well as at its beginning. A similar set of experiments were made with the guinea-pig, and the result was analogous; but the precise relation of the losses in each medium could not be determined on account of the difficulty of making allowance for the loss sustained by excretion. In conducting these inquiries several niceties require to be attended to, for which we must refer to the work. The transpiration is also materially affected by the state of the air as to repose. Reasoning would lead to this doctrine; but Dr Edwards has also established it on facts. It is difficult or even impossible to determine the actual degree of power which brisk agitation of the air possesses over a state of repose, because it is impossible to preserve it in a state of perfect repose, without using such means as must alter materially its hygrometric condition. But the difference is certainly very great; for even in the gentle agitation of a chamber, the loss in a given time is considerably greater than in confined air kept dry by quicklime, and, of course, in a state of almost perfect repose.

IV. We shall now pass with our author to the fourth and last great division, which treats of the Influence of Physical Agents on Man and Vertebrated Animals generally. The reader who has paid attention to the criterions by which Dr Edwards determines the influence of physical agents on the lower animals, will easily see that a similar mode of investigation cannot be applied to the human species. A different mode has therefore been adopted in the present division. Having examined the relations subsisting between physical agents and various tribes of the lower animals, and having ascertained, that, with certain unimportant exceptions, these relations are the same in all of them, he extends the fact by analogy to man and other tribes of animals not expressly examined, and supports this analogical reasoning as he proceeds, by referring to occasional observations and detached experiments published by others, or made expressly by himself.

The first five chapters of this division treat of Animal Temperature, and the power of maintaining and producing it.

1. The first object of his attention is the relative power of producing heat possessed by the human species at different ages. From the *data* ascertained in the former part of his inquiries he infers, that, in the infant state, man must possess the power of maintaining his temperature, unless his birth is premature; for in the former case he is born perfect, in the latter imperfect. Accordingly, there can be no doubt that mature children can maintain their temperature in mild air. Yet the power is not quite so perfect as in after years; and we should be disposed to draw this inference from a fact overlooked by the author, namely, that children, though born more perfect than those young animals which cannot maintain their temperature, are yet less perfect than those which can do so. The imperfect power possessed by children of maintaining their temperature cannot be determined by positive experiment; but it may be inferred from the circumstance of their actual tempature being inferior to that of adults. Dr Edwards found the temperature of twenty adults to vary between 96° and 98½°, while that of ten children, none of whom was above ten days old, varied between 93¼° and 96°; and that of a seven months' child, two days after birth, was only 89½°.

2. Such being the fact, therefore, with regard to young animals generally, what effect will this deficiency in their power of producing heat have over their health and life at different ages? In other words, what is the influence of cold on the mortality of different ages? When kittens, puppies, or sparrows, are exposed to air, about the mean annual temperature, or somewhat lower, in which circumstances their heat will speedily fall to 68°, or even 57°, they become stiff, almost insensible, and unable to move. If they are exposed in this state to warm air, they speedily regain their heat, and with it their sensibility and power of motion; nor do they seem to have suffered any injury by the experiment. Nay, it may be repeated several times without apparent harm, though doubtless their

health must be affected ultimately. Farther, they may be kept in a state of torpidity, with their bodily temperature at 68° or even 62°, for two or three days, without sustaining any injury, provided the external cold be not too severe. But the case is very different with adults of the same species. It has been shown that adults resist the action of cold much more effectually; but if their temperature be once reduced by it, the consequences are also much more prejudicial. At first, indeed, they seem to recover as readily as the young; but Dr Edwards found that the greater part of the birds on which he made the experiment perished a day or two afterwards. He does not appear to have made the same experiment on quadrupeds.—The author has traced very beautifully, at the close of the chapter, the final causes of this difference in the effects of cold.

3. Although the body soon recovers its temperature after having been cooled by exposure to cold air, it is an important fact, that its faculty of producing heat is for some time impaired; for a shorter time is required to effect again the same reduction of temperature in air of the same degree of cold; and if the reduction be repeated several times, after each repetition a longer period is required to restore the temperature. Hence Dr Edwards remarks, the sensation of cold which is apt to continue some time after exposure to severe cold and subsequent restoration of heat, is not altogether a morbid sensation, as is generally thought, but really arises from a deficient supply of internal heat, and a consequent tendency in the body to cool again.

4. On the other hand, prolonged exposure to heat, after exposure to cold, has a tendency to restore the power of producing heat. A practical deduction from this fact is the rule which experience has long ago taught the natives of cold climates; viz. that the best way to enable the body to endure extreme degrees of cold, is to keep it previously well warmed.

5. The influence of the seasons may be deduced from the facts and principles laid down in the previous chapters. During the heat of summer, the greater number of the warm-blooded animals gradually acquire an increased power of producing heat, which is manifested as winter approaches; while the cold of winter gradually impairs the same power, so as to prepare the body for the less demand on this function during the summer. These salutary changes, however, do not occur in all animals. The hybernating animals never acquire a sufficient power to resist the cold of winter. Other animals too, which do not hybernate, nevertheless have but an inferior power of producing heat, and consequently must remedy the deficiency by artificial means. The mouse, according to Dr Edwards, is thus circum-

stanced. Unlike most other mammiferous animals, it builds warm nests for the winter; and he has found, that in that season it cools very rapidly when exposed to moderate cold. Although man ranks with the most perfect animals in these respects, it is highly probable that all persons do not undergo with the seasons the same salutary changes in the faculty of producing heat. There is a difference among individuals in their capability of enduring cold, which cannot be attributed to mere sensation, or the simple force of habit. It probably depends in a much greater degree on differences in the power of producing heat; and this idea derives some confirmation from the fact, that persons very sensible to cold, if they have been much exposed to it, are warmed again much more slowly than others.

The five succeeding chapters of the fourth section relate to the influence of physical agents on the respiration of man and vertebrated animals generally.

6. When deprived of air, man speedily perishes like the tribes of animals formerly noticed. It is uncertain whether the progress of asphyxia is less rapid in infancy than in the adult age. On the one hand, children at birth are somewhat allied to those young animals which are born imperfect; on the other hand, they possess pretty perfectly the power of maintaining their temperature. On the latter account, it is probable that the progress of asphyxia will be very nearly the same at all periods.—Although the young of some animals can live a long time without access to air, Dr Edwards remarks, that the voluntary motions of the body cease in all about the same time. He has known some puppies live 54 minutes under water; yet their voluntary motions ceased in four minutes.—Habit has very little effect on the power of enduring the privations of air. The longest period during which the best divers of Paris, who are very expert, can remain under water, is three minutes. It is probable that temperature has some influence on the duration of the asphyxia of vertebrated animals generally. We have seen that it affects materially its duration in young animals born imperfect; Dr Edwards adds, in the present place, sufficient proof that it also affects very much the asphyxia of adult hybernating animals in their torpid state; but the duration of asphyxia in other adult animals is in all circumstances so short, that the agents which modify it can rarely be appreciated. It is farther probable, that in all animals the circulation of venous blood has some effect in prolonging life during the privation of air. This has already been shown with respect to cold-blooded animals; and the author now extends the fact to such young animals as are born

imperfect. The kitten lives from 30 to 50 minutes under water, but if its heart be previously cut out, it survives scarcely half that time.—The differences produced by temperature in the duration of asphyxia, depend partly on its power over the circulation, partly on its influence on the nervous system. An increased temperature must tend to abridge life during asphyxia; for whatever quickens the circulation has this tendency. But the influence of temperature is manifest, even when circulation is suppressed. A kitten, with the heart cut out, lived 13½ minutes in water at 68, 7 minutes in water at 104, and 5 minutes in water at 32°; which facts show that a low and high temperature equally abridge life by exhausting the nervous energy.

7. The consumption of the air by respiration is very different in rapidity in different tribes of vertebrated animals. Taking, for example, the extremes of the scale, the author finds, that if a yellow-hammer lives an hour in a limited quantity of air, a frog of the same bulk will live in the same quantity three or four days. Yet the degree to which the air is consumed, is pretty nearly the same in both. The cause of the difference is partly the greater proportional size of the lungs, and greater extent of the air-cells, partly the greater rapidity of the circulation, and partly the greater abundance of red globules in the blood. Young animals born imperfect consume less air than adults; and according to De Saissy, hybernating animals consume much less in their torpid state than during summer.

8. As to the combined action of air and temperature on the respiration, we have seen that in cold-blooded animals the decrease of the one compensates for a diminution of the other. If the access of air be in any way impeded, these animals are enabled to live provided the temperature of the surrounding medium be reduced; and *vice versa*. Our author thinks the same facts may be inferred, with regard to young warm-blooded animals, from some experiments of Legallois; who found, that although young puppies perish almost immediately after the section of the pneumo-gastric nerves, in consequence of the obstruction to the entrance of the air caused by the closing of the glottis, yet they will live a considerable time in this state if they are rendered torpid by cold. Dr Edwards infers, that the same compensation may be effected in adults, and quotes, in proof of this, the practice adopted with advantage in cases of syncope, asthma, and in asphyxia produced by respiring an impure air. In all such cases the practice is to expose the whole body to cool air, and even to dash cold water on the face and breast. We question, however, whether the good effects of this practice arise from the cold which it exites, reducing the quantity of air required for

respiration. Another much more probable mode of action might be pointed out; but the plan we have adopted in this analysis, will not admit of such deviations.

9. This chapter is devoted to a short account of the effect of temperature on the respiratory movements, and of the reasons why both a very high and a very low temperature have a tendency to accelerate respiration. It is not susceptible of analysis, and, besides, does not contain any doctrines of very great importance.

10. The influence of the respiratory movements on animal heat is beautifully shown in this chapter, from the effects of irritants on hybernating animals and birds that are born imperfect. M. de Saissy found, that by applying mechanical irritants to hybernating animals in a state of torpor, and exposed to a cold atmosphere (at 34½°), the respiration was soon accelerated, sense was gradually restored, and the animal temperature rose in a few hours from 40° to 80.°5, 86°, or even 97°. It is singular too, that, by a wise provision of nature, the very cause which produces their state of torpidity, becomes the means of rousing them if carried to an excess, by accelerating the respiration. If these animals in a state of torpor be exposed to a cold of 25°, the breathing soon becomes quicker, the senses return almost as soon as under the influence of mechanical irritation, and in two or five hours from the beginning of the experiment, their heat will rise from 40° to 97°. But the cause which excites this change is incapable of maintaining it, and they speedily become again torpid, and will ultimately perish. Dr Edwards has observed an analogous phenomenon in young birds that are incapable of maintaining their temperature. If those, which are somewhat advanced in age, be exposed to cold, their temperature first decreases, then their breathing becomes quicker, and in no long time their temperature is restored in part or altogether.

11. The following chapter, on the transpiration of man and the vertebrated animals, and the influence of physical agents on it, contains few original experiments, but consists of an elaborate detail of the statements of former observers, and a comparison of them with each other, and with the general laws, respecting the transpiration of the lower animals, established by the author in the foregoing part of the work. Dr Edwards considers himself entitled to assume from the uniformity of the results obtained in his experiments on the lower animals, that in man also the loss sustained by transpiration will diminish progressively with the time; a fact which, he suggests, has been quite overlooked by former physiologists, in the re-

searches they have made concerning this function.—Sanctorius thought that transpiration was diminished for some hours after eating. Our author endeavours to show that this aphorism is deduced by reasoning, not established on experiment; and quotes from the inquiries of Keill, and of Sanctorius himself, several facts which prove the contrary.—With respect to the influence of sleep, he suggests that we should anticipate from his experiments on animals a diminution during this state; yet both Sanctorius and his successors have alleged that it is increased, and he himself has made some observations to the same effect.—There is every reason to believe, he continues, that differences in the hygrometric state of the air will affect materially the transpiration of man as well as of the lower animals. On the latter, the utmost hygrometric variation we can command, causes a difference of six to one in the proportional loss; and in man, the difference will not be less. Few of the numerous physiologists, who have made experiments on transpiration, have paid much regard to this condition of the air; and none have estimated its power to be considerable. Sanctorius thought that agitation of the air diminished the loss by transpiration, and other observers have confirmed that opinion. Dr Edwards shows that the contrary must be the case, and that these physiologists must have erred, in consequence of neglecting the modifications which arise from other causes.—Atmospherical pressure will probably affect the transpiration of man, as it does that of the lower animals; but as its variations are trifling, so the differences arising from these variations must be slight.—The agents hitherto noticed affect only transpiration by *evaporation*. But this function is twofold; a loss is also sustained by *transudation*, or a variety of true excretion. It is difficult to discover the precise relation subsisting between the two kinds of transpiration, by direct experiment on man; but it may be inferred from the uniformity of the results obtained with all the lower animals. The ratio of transudation to evaporation, in cold-blooded animals, as determined by the respective losses in dry air, and in that saturated with moisture, is one to six; and in all of these animals the ratio is the same, however the absolute loss may differ. The same ratio holds with such warm-blooded animals as can be subjected to accurate experiment; and therefore it may be safely adopted as holding with respect to man also. We are now prepared to estimate the influence of temperature on transpiration. The loss by *transudation* is sensible at the lowest temperatures; it can never be altogether suppressed, as many improperly imagine. Some cold-blooded animals lose in a few hours a thirtieth part of their weight by transudation in air at 32°. Betwixt this temperature and that of 104°, the loss from the same cause becomes five and a half times greater; but very little increase is observable till the temperature rises above 68°. It is probable that in man the effects of temperature are nearly analogous. A healthy person in a state of repose never perspires till the temperature rises above 68 or 70. But a few degrees of increase above that point augments the perspiration very much, and it goes on increasing as the external temperature rises. The loss by *evaporation* is differently influenced. It must increase steadily with the temperature from the lowest degree upwards. But when at length the temperature reaches that degree at which the transudation is so much increased that it appears in the form of sweat, a farther increase of heat cannot produce a corresponding increase in the loss by evaporation. For, as the body is covered by moisture, the agency of the air in producing evaporation is exerted, not on the body itself, but on the product of transudation.—Transpiration takes place partly from the skin, partly from the lungs. Lavoisier and Séguin have estimated the relation of the former to the latter to be as 2 to 1. Pulmonary transpiration consists only of evaporation; transudation can scarcely take place at all from the lungs, as the matters disengaged must be all in the gaseous state. The matter transpired from the lungs is chiefly water, partly carbonic acid.—When the body is immersed in water, it is evident, that the only kind of cuticular transpiration which can have existence is that by transudation.

12. Our author next proposes to inquire, whether the human body is capable of absorbing water when immersed in that fluid, or in air in which it is dissolved. Haller and other physiologists have alleged, that the human body gains weight when immersed in water; but of late years more reliance has been placed, particularly in France, on the researches of Séguin, who, during his numerous experiments on this subject, was never able to procure any satisfactory proof of absorption. Dr Edwards considered this failure very singular, after the undoubted proofs of absorption he himself had procured in the case of some cold-blooded animals; especially as he found it to exist, not only in frogs and others of delicate skin, but likewise in fishes, lizards, and other animals, whose bodies are covered with a much thicker integument than that of man. He suggests, therefore, that Séguin had been led into error by not attending to the circumstances in which his experiments were made. In the first place, absorption in water is never so great as to increase the weight of the body, unless the body has been previously

allowed to transpire so as to be reduced below its point of saturation;—a precaution which the French physiologist probably never employed, as he was not aware of its importance. But, secondly, Séguin, misled probably by the great and sudden increase of *transudation* at temperatures above 70°, assumed that none took place below it; whereas our author has proved, that even at 32° the loss from this cause is considerable. Now, in the experiments made by the former at temperatures between 52° and 70°, the total loss sustained in the bath was generally accounted for by the quantity of pulmonary transpiration alone. But some loss must have been sustained by cuticular transudation also; and consequently this loss must have been compensated by absorption. It is highly probable, therefore, that the human skin absorbs water when immersed in it.

13. The absorption of water by the body, when placed in air loaded with humidity, rests on evidence nearly of the same nature. Even in air saturated with moisture, the lower animals generally lose weight progressively; but, in some cases, Dr Edwards observed, that they preserved it uniform for a considerable time; which is sufficient to show that the transudation must have been compensated by absorption. In one instance he even remarked, that a snake, after losing weight in moist air, recovered it again. Among the numerous experiments which have been made on man, few instances can be found of the absorption of humidity from the atmosphere. Yet some instances are on record, and many others would probably have occurred, if the experiments had been oftener conducted when the body was in circumstances favourable to absorption. Keill relates, that his body gained one night during sleep 18 ounces in weight; and Lining remarks, that on one occasion during diuresis, induced by drinking punch, the absorption exceeded the transpiration by 8¼ ounces in two hours and a half. We should conceive it by no means improbable, that if the body be in favourable circumstances, the absorption in humid air will even exceed that in water.

14. The next chapter treats of animal temperature, the extent to which it may vary under the influence of physical agents, and the means by which that influence is modified. The human body, and that of animals, can support an external temperature much above their own. Delaroche and Berger found, that in dry air the cat, rabbit, pigeon, yellow-hammer and frog could endure for half an hour, without much inconvenience, a temperature of 109° or 113°. If the time was prolonged, however, they were evidently distressed; the breathing became rapid, and, on being removed from the stove, they continued

much exhausted for an hour afterwards. If the temperature was much higher they soon perished; when the heat was at the beginning 133°, and rose gradually to 149°, all of them, except the frog, died within two hours. It is probable that man could not long sustain a greater heat; although there are instances of the human body having endured for a short period a heat of 210°, 224°, 240°, and even 260° If the air is loaded with moisture, the sensation of heat is much more intense, and the temperature which can be supported is considerably less. Delaroche and his coadjutor found, that they could not bear above 8 or 12 minutes the temperature of a vapour bath, beginning at 99°.5, or 106°.25, and ending at 124°.25, or 128°.75; but in dry air they could support a far higher temperature much longer. In water itself, the temperature which can be borne is even less. The frog can bear for five hours and upwards a vapour-bath at 104°; but in water at the same temperature it dies in two minutes at the utmost, even though its head be kept above the surface. There are few accurate observations on this subject relatively to man. Lemonnier found, that he could not bear above eight minutes a water-bath at 113°; and Dr Edwards appears to have been unable to discover any instance on record of the human body having been subjected to a higher temperature. Although man and other animals have the power of maintaining their temperature under great vicissitudes of heat, and even of preserving it beneath that of the medium in which they are placed, it is nevertheless increased by prolonged exposure to very high temperatures. The most accurate facts on this subject are ascertained by Delaroche and Berger, who found, that, after remaining 15 or 17 minutes in a vapour-bath at 120°, their own temperature rose betwixt 2½ and 5½ degrees; that warm-blooded animals, left till they died in a vapour-bath at various temperatures between 122° and 200°, had always acquired nearly the same increase at the moment of death, namely, betwixt 11¼ and 13 degrees; and that the utmost temperature which, in the same circumstances, cold-blooded animals can attain before they die, is 105°.—At these high temperatures, there exists a remarkable exception to the general rule regarding the influence of moisture on transpiration. It is greater in moist than in dry air, and still greater in water. The reason is obvious. At high temperatures, the only kind of transpiration which can take place is that by transudation, and this is increased most by the contact of that medium which imparts the greatest heat. But although there exists no transpiration by evaporation, evaporation nevertheless takes place, namely, of the fluid discharged by transudation; and it is owing to the refrigerating power of this evaporation that the body is enabled to withstand so powerfully the tendency of the surrounding heat. This power, therefore, should be purely a physical one. Accordingly, Delaroche and Berger found, that a porous earthen-ware vessel filled with water, a sponge soaked with it, and a frog, when kept two hours in a stove heated to between 126° and 142°—all attained nearly the same temperature, namely, about 98°. Warm-blooded animals, in similar circumstances, will of course have a higher temperature, as in their case there is an additional source of heat; but the difference would appear from the experiments of the same physiologists to be very slight.—Evaporation being the cause of the cooling of the body at high temperatures, it follows that this cooling will be greatest in dry air; and hence it is that the body can endure hot, dry air better than air loaded with moisture. But what will be the comparative effect of dry and moist air in cooling the body, when the temperature of the air is beneath that of the body? Air at low temperatures will cause more evaporation when dry than when moist; but, on the contrary, moist air has a greater conducting power. It is generally believed, that this increase in its conducting power more than counterbalances the effect of diminished evaporation; and that, *cæteris paribus*, moist air at a low temperature cools the body faster than dry air. This idea, however, rests chiefly on difference of sensation; which is a very fallacious criterion. Dr Edwards has attempted to settle the point by experiments on those young animals which cannot maintain their temperature at birth; and he finds, that the mean of ten experiments on young sparrows gives a ratio of 65 to 67, the balance being in favour of air loaded with moisture. Of course, the difference is so very trifling, that the two states may be assumed as of very nearly equal power.—The comparative effect of repose and agitation on the cooling of the body has not been ascertained by experiment; but we may be certain that the difference is very great. Our author quotes, in proof of this assumption, a remarkable fact observed during Captain Parry's voyage to the Arctic regions, namely, that when there was a breeze, with the temperature of the air at zero, greater inconvenience was felt from the cold, than in still air at 51 degrees lower.

15. Dr Edwards next attempts to estimate the influence of Light as a physical agent on man and the vertebrated animals. He has not entered fully, however, into this difficult and embarrassed subject. He shows by experiment, that light has a considerable influence on the interval required for the birth of the tadpole from the egg, and likewise on the duration of the tadpole state; but he does not appear to us very suc-

cessful in his attempts to show, that the growth and perfection of the human body are promoted by the same agent. The comparative facts adduced in support of that idea may be explained on much more probable grounds.

16. We now come to the most elaborate and most interesting part of the work; namely an inquiry into the changes produced in the air by respiration. It is well known, that, notwithstanding the multiplicity of able experimenters, who have employed their zeal and talents on this subject, the chief facts and doctrines regarding it have been long enveloped in doubt and obscurity. Every experimenter has found that part of the oxygen is lost, and that carbonic acid appears; but upon no other point are they agreed. It is uncertain, whether the quantity of oxygen which disappears is equivalent to that contained in the carbonic acid which is formed; it is uncertain whether the azote of the air is of any other use than merely to dilute the oxygen; it is not even determined, whether the carbonic acid is an exhalation from the blood, or a mere combination in the lungs of the inspired oxygen with carbon. Of late, physiologists have appeared willing to concur with the most recent researches on the subject made in 1808 by Messrs Allen and Pepys; and to believe with them, that the lost oxygen is exactly sufficient to form the carbonic acid; that the appearance of the carbonic acid is owing to a simple chemical change of the inhaled air, effected in the lungs; and that the azote of the air undergoes no alteration. But the numerous contradictions to be found among the experiments of different chemists led Dr Edwards to doubt the accuracy of the prevailing opinions, and consequently to examine the whole subject anew and more extensively: and the results he has obtained are such as will produce a material change in our views of the physiology of respiration, while they account amply for the discrepances existing among former observers. His success in this difficult and well-trodden path has arisen partly from superior precautions and nicety of manipulation, partly from the extension of his experiments to various tribes of animals, and partly from his attention having been turned to the changes effected during various states of the breathing, and under the influence of various physical agents. The chapter consists of five sections.

a. On the relation of the lost oxygen to the carbonic acid formed.—To determine this point, he introduced a puppy into a glass vessel containing 150 *centilitres*[*] of air, and left it there five hours. Absorption was evident; the volume of the air was diminished by 9.3 c.; and 17.86 c. of carbonic acid were formed. But was this diminution of the volume owing to the direct absorption of oxygen, or to the absorption of carbonic acid formed from the oxygen? The former is shown to be the fact by two circumstances. In the first place, the diminution of volume takes place from the very commencement, when there is hardly any carbonic acid present; and, secondly, it does not increase with the time. In the experiment just related, the proportion of the absorbed oxygen to the whole of the oxygen which disappeared, is about 1 to 3. With other animals the proportion is different; with the frog it is 1 to 3.4; with the guinea-pig 1 to 5, and with the yellow-hammer only 1 to 5.6; and in some individuals of these species the oxygen lost is almost inappreciable. No one need be now at a loss to comprehend why the statements of former physiologists have been so different from one another. In fact the differences existed in nature; they did not arise, as was generally thought, from experimental errors.

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[*] Equivalent to the volume of somewhat more than three English pints.

b. On the relation of the azote inspired and expired.—On this point the opinions of authors are even more variable. Some have found the proportion of azote to be unaltered, others have found it diminished, and others increased; but, on the whole, the prevailing opinion coincides with the original statement of Lavoisier, confirmed by Allen and Pepys, that it is not altered. Dr Edwards, however, has shown, that all the three opinions are correct. In the experiments mentioned in the last section, which were made in summer, he almost always observed an augmentation in the quantity of azote: sometimes, indeed, it was so trifling, that he could not but assume it as unchanged; but in many other instances it was so great as to place the fact quite beyond the possibility of doubt, and occasionally it almost equalled the whole bulk of the animal. Such continued to be the result of his inquiries till the close of October, when he observed on the other hand a sensible diminution of the azote; and the same result was generally obtained throughout the whole winter and beginning of spring. The quantity absorbed was very various, sometimes almost inappreciable, sometimes equivalent to the extreme of exhalation observed in the last set of experiments. He considers it highly probable that other causes besides season influence the production of these phenomena. On a few occasions he observed exhalation in winter, and absorption in summer. In young animals he almost always found exhalation.

c. On the exhalation and absorption of azote.—Azote therefore being sometimes absorbed and sometimes exhaled, it became an object worthy of inquiry, whether the phenomena are simple or compound, that is, whether exhalation alone takes place at one time, and absorption only at another; or whether the two processes are carried on simultaneously, so that the result is merely the difference between them. Dr Edwards decides in favour of the latter doctrine; and founds his opinion on the phenomena observed when some other gas is substituted for the azote. Messrs Allen and Pepys observed, while making experiments for a different purpose, that when an animal was made to breathe impure oxygen, containing only about five per cent. of azote, a quantity of the latter gas was exhaled which surpassed the volume of the animal; the same result was likewise obtained when it breathed an artificial air in which hydrogen was substituted for the azote; and Dr Edwards observed the same fact when he repeated the experiment on animals. Now, if exhalation was the only process that went on in those cases of ordinary respiration in which azote is exhaled, then, on removing the azote inspired, the quantity exhaled ought not to increase. Whereas in these experiments it was very much increased; for, under similar circumstances, Allen and Pepys found that respiration in common air did not alter the proportion of azote at all. Hence exhalation and absorption take place at the same time; and the phenomenon observed is the result of the difference between them.

d. On the origin of Carbonic Acid in Respiration.—There are evidently two ways of viewing the production of the carbonic acid. Either it is exhaled from the blood; or it is formed by the union of the oxygen of the air with the carbon of the blood through the membranes of the pulmonary air-cells. The latter opinion was adopted by Lavoisier, and has been generally recognised by succeeding physiologists. The question is to be determined by experiments analogous to those stated under the last section. That is, let the air inspired be deprived of its oxygen, and if carbonic acid be nevertheless found in it (beyond what can be produced by the small quantity of oxygen left in the air-cells at the beginning of the experiment), then it is in part at least an exhalation from the blood. This experiment can be performed only on a few animals; those namely, which can exist some time in an atmosphere deprived of its oxygen, and which likewise do breathe in it. Dr Edwards commenced with the frog-tribe. These animals must be taken for the purpose in the winter or spring; otherwise they die too soon. Having expelled the air thoroughly from the lungs by squeezing its flanks, he kept a frog eight hours and a half in 153 centilitres of the purest hydrogen; and he found, by analysis, that 2.97 centilitres of carbonic acid were formed, or nearly as much as the volume of the animal. This experiment was repeated several times, and occasionally the volume of gas formed was even greater. It was next tried upon fishes, and the result was the same; and finally upon snails, with similar success. Spallanzani, too, had previously established the fact with regard to the last mentioned animal. Our author considers that he would have been perfectly well entitled to infer from these experiments on cold-blooded animals, that the same fact holds also with respect to those of warm blood, as the chemical changes by respiration are in other respects completely analogous throughout the whole range of animated beings. He likewise applied them, however, to those young warm-blooded animals which can live some time without oxygen. A kitten, three or four days old, was left for nineteen minutes in 146 centilitres of pure hydrogen; and he found that it had formed in that time nearly two centilitres of carbonic acid, or twelve times as much as could arise from the oxygen left in its lungs at the beginning of the experiment. It appears, then, that the carbonic acid of respiration is produced at least *in part* by exhalation from the blood. But does the whole of it arise from the same source? This question can only be answered by comparing the quantity in the air with that formed in a gas which contains no oxygen. Now, such an experiment is not easily made; because an animal must be chosen which can not only live some time in a disoxygenated atmosphere, but likewise breathe with equal freedom as in common air. The frog is the only animal which on these accounts appears fitted for the purpose. As far as the phenomena observed in its respiration will justify the conclusion, the carbonic acid is *altogether* an exhalation from the blood; for in similar circumstances as large a quantity is formed in hydrogen as in common air. Nay, the difference is rather in favour of respiration in hydrogen.

To conclude, the phenomena of respiration in respect to the inspired air are the following. All the oxygen which disappears is absorbed by the blood; carbonic acid is exhaled; nitrogen gas is also exhaled, and it is likewise absorbed. The carbonic acid is sometimes equivalent to the oxygen which disappears, sometimes it is less; and the azote exhaled is sometimes inferior, sometimes equal, sometimes superior, to that absorbed.

17. The author finally winds up his Treatise by applying the foregoing inquiries to explain various phenomena in the

physiology and pathology of man. Under this head he first attempts to explain some of the most remarkable facts in the history of animal heat.

We have seen, that when the faculty of producing heat is in any way diminished, either the functions become torpid, or this is prevented by an increase in the respiratory movements and in the circulation. Although the former effect is confined chiefly to the hybernating animals, and the young of some others which do not hybernate, yet many reasons lead to the conclusion that even the adults of all classes of animals are occasionally liable to undergo the same change. Some hybernating animals are so apt to become torpid, as to fall into that state even from the slight decrease of their power of producing heat which takes place during sleep; and it is not unreasonable to infer, that the same may be the fact with individuals of other classes, and even with man himself. The effect of cold in producing lethargic sleep in man is well known; the degree of cold required is very different with different individuals; and perhaps there may be some individuals who will become lethargic even at moderate temperatures, if other causes concur to diminish their power of producing heat. Thus we are led to admit the possibility of those cases of lethargy or prolonged sleep which have been so often recorded, and so frequently questioned.

On the other hand, the phenomena that occur in those animals in which the diminished power of producing heat is attended with acceleration of the pulse and breathing, appear to throw some light on the progress of the febrile paroxysm. The first stage of a fit of ague is exactly similar to the state into which young puppies and kittens are thrown when removed from their mother even into temperate air. And that this arises in the former as in the latter case from a want of the power of supporting temperature, is rendered probable by the sensation of intense deep-seated cold which accompanies the cold stage of the fit, and by the great refrigeration of the body which takes place if cold is applied at that time. But ere long the continued action of the cold quickens the pulse and the breathing; and through these means the production of heat is gradually increased; nay, it may exceed the natural standard: And this effect is not temporary; because we have seen, that, whenever any extraordinary means are brought into play to increase the productive power, their influence continues a considerable time after their application has ceased. If, instead of leaving the restoration of the productive power to nature, artificial means be early used to increase it, then the subsequent hot stage will be curtailed, or even prevented altogether; and such Dr Edwards considers to be the mode of action of the vapour-bath in cutting short ague.

It has been shown, that, at temperatures below that of warm-blooded animals, the cooling of the body is not affected differently by the dryest and moistest condition of the atmosphere. This fact appears incompatible with the different sensations produced by dry and damp air at the same temperature. Dr Edwards considers the reason to be, that dry cold air acts merely on the surface of the skin, while moist air has the peculiar power of exhausting the faculty of producing heat; and he appeals, in support of his opinion, to the different qualities of the sensations produced by each. In dry air, the sensation of cold is superficial; and when the influencing power is intense, it causes rigidity and torpor; but in moist air, the sensation is deep-seated, and its consequences are nearly the same as those mentioned in the last paragraph. Such being the state of the case, we can be at no loss to understand why the latter condition of the atmosphere is the most dangerous to the health.

The capability which the body possesses of supporting changes of climate, has been generally ascribed to the difference of evaporation under different temperatures. This circumstance has no doubt some effect; but Dr Edwards considers it much more trifling than one would be apt to suppose; for the temperature even of the frog, whose evaporation is much greater than that of any warm-blooded animal, follows very closely the changes of the season. A much more effective agent is a change in the power of producing heat, which becomes greater on a change being made to a colder climate, and less if the change be to one that is warmer. But notwithstanding these regulating powers, the animal temperature, contrary to the common belief, does vary somewhat from season to season, and between one climate and another. The greatest variation observed by Dr Edwards, betwixt summer and winter, was in the case of the sparrow; the mean of several experiments gave 105°.33 for February, 107°.5 for April, and 110°.75 for July. Dr John Davy informed the author that he found the temperature of the human body one or two degrees higher at Ceylon than in Europe.

Under the influence of disease, the power of producing heat is often greatly augmented. The author has given, on the authority of Dr Prevost of Geneva, the highest temperature which has yet been observed in man; he found it in a boy 12 years old, labouring under tetanus, 110¾. *

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* During the last six years, we have had abundant opportunities of ascertaining accurately the increase of heat which may take place in the most acute febrile dis-

The good effect of the cold bath and cold affusion in febrile diseases is attributed by the author chiefly to their power of diminishing the faculty of producing heat; that of cold sponging and ventilation depends rather on the increased evaporation counterbalancing the tendency of the productive faculty.

He considers that the peculiar symptoms, which occur at great heights above the level of the sea, are owing not altogether to the diminished supply of air, arising from its tenuity, but likewise in a great measure to the increased evaporation from the lungs, caused by its excessive dryness. Hence arises the intolerable sensation of thirst; and hence, too, it happens, that the oppression of the breathing is much relieved when clouds pass over the body. For the same reason, some delicate individuals suffer considerably from oppressed breathing, in apartments heated by stoves, in the cold northern climates; and their sufferings are relieved if the air is rendered moist by vessels of water distributed throughout the apartments. Animals can endure a very great rarefaction of the air, if the quantity is limited so as to be easily loaded with moisture, and if they are not subjected at the same time to violent exercise. Guinea-pigs appear to be on the point of suffocation only at a pressure of 9.1 centimetres (0.60 inch), and the yellow-hammer at 13.5. (0.81).

In many febrile diseases, a great source of suffering, and probably of danger to the patient, is the increased transpiration from the lungs and skin. It is probably an object of much greater importance than is generally thought, to counterbalance its effects by external means. Is not this the cause of the great relief felt by such patients in the inhalation of watery vapour?

The statements given in the body of the work, regarding the low power of producing heat possessed by young children, and the probable differences in the same respect which exist among adults, might lead to many interesting rules respecting regimen as to clothing. They will tend to do away with the notions which some persons have entertained of the propriety of inuring very young children to cold, and to establish the necessity of paying the greatest attention to preserve their temperature by

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eases of this town. The observations which have been made by ourselves, and by others on whose accuracy we could rely, in cases of continued fever, scarlatina, measles, small-pox, &c. have been several thousands in number; and we rarely found it above 105°, in perhaps half a dozen instances 107°, and once only 107½. These high temperatures were determined by a thermometer, which was carefully corrected by the usual means. The necessity of this precaution is even more urgent than may be imagined. We have examined within these few years, eleven thermometers graduated by some of the first makers in London, Paris and Edinburgh, and have found them all to vary between 0.5 and 4.0 of the truth.

sufficient clothing. They likewise show, that with some individuals a change of dress is indispensable as the seasons change, while others may safely clothe themselves in the same manner throughout the whole year.

All young animals can endure respiration in confined or impure air much better and longer than adults. Accordingly, infants may suffer, without much danger to life, diseased states of the lungs, which would be fatal to adults. It is probable, too, that as adults differ much from one another in their power of producing heat, so they likewise differ considerably in their power of enduring confined respiration; and this fact would in part explain, why, in apparently similar circumstances, morbid alterations of the lungs of the same extent are fatal to some, and not essentially injurious to others.

It may be worth while to inquire, what practical hints may be derived from these views respecting respiration, for the treatment of diseases of the lungs. In such diseases, one of the objects should be, to alter the constitution to a state in which it can support confined breathing. This may be done, by diminishing the quantity of the blood, and the number of its red globules. For we have every reason to believe, that the changes effected in the lungs are proportioned in degree to the proportional quantity of blood in the body, and its relative richness in red globules. Such at least is very remarkably the fact in different tribes of animals. Prevost and Dumas have found that the blood is most watery in the cold-blooded animals, richer in the mammalia, and richest of all in birds. The required change may be effected partly by rigid abstinence; but this mode is too slow. The most effectual is the diminution of the quantity of the blood by venesection. The lost volume is soon supplied by the absorption of water, and a thinner blood is formed; as Prevost and Dumas have established by actual observation. These views would afford a beautiful explanation of the effects of blood-letting in relieving many of the symptoms both of pulmonary and of other inflammatory disorders; but our limits will not permit us to deviate from our author's course.

The last application we shall notice is one probably of some importance, relative to the treatment of asphyxia. Dr Edwards has found, that when the respiration becomes confined from any cause, so as to endanger life, the demand upon this function may be diminished and life in consequence prolonged, by diminishing the temperature. Hence he is inclined to disapprove of the unlimited use which some persons make of external heat in the treatment of asphyxia. The temporary application of brisk heat may be advantageous by the sudden excitation it causes; but too long a continuance of it, especially in those varieties in which the bodily temperature is not materially diminished, and, above all, after the breathing has been partially reestablished, may be injurious by increasing the demand of the constitution for a still freer supply of air. He considers these views peculiarly applicable to the treatment of still-born children; and suggests, that the immersion of the body in warm water, so often practised with success, ought never to be continued after the breathing has begun.

Appendix on Electricity.

This Appendix is of a very different cast from Dr Edwards's share in the work. It is not an investigation of the effects of electricity as a physical agent on the great animal functions: it is a theoretical explanation of the mode in which accumulated electricity acts on life, and of the relation which subsists between the nervous energy and the electric current.

Our readers are aware, that, according to the opinions at present received by natural philosophers, electricity may exist in bodies, under two very different conditions; first, as accumulated and at rest on their surface; and, secondly, as forming continual currents through their substance. The former is designated by the name of Electric Tension, and is exemplified in the state of the conductor of an electric machine when excited in the usual manner: the latter is termed the Electric Current; and we have the best example of it in the state of the conductors of a charged galvanic trough, when the conducting circle is complete.

The most remarkable phenomenon in the action of the electric tension on bodies generally is its tendency to separate their particles to a distance from each other. This tendency is sometimes exemplified in its action on animal substances. Thus, when passed through a drop of blood, it instantly gives the globules a fringed and ragged appearance; and the same effect is produced on infusory animalcules when a spark is sent through the fluid which contains them. Now, when a strong shock is transmitted through the body at large, those tissues, say MM. Prévost and Dumas, will sustain the greatest injury of the kind, which are the best conductors of electricity; namely, in vertebrated animals, the tissue of the nerves. And in fact there can be no doubt, that the nervous system undergoes the most material injury when the body is struck by lightning. The power which the nerves possess of conveying impressions is often much impaired, or even altogether destroyed; an effect which may be ascribed to the electric fluid having displaced the nervous globules, and jumbled them with the fatty particles which separate the fibrils from one another.

With regard to the electric current, its most remarkable property is the attraction it establishes between conductors which it traverses in the same direction, and the repulsion which exists between them when it passes through them in opposite directions. To the first of these phenomena MM. Prévost and Dumas think they have observed something closely analogous in the phenomena of muscular contraction. We shall present their ideas to the reader in detail, as they are both new and interesting and ingenious. When a nerve is examined with the aid of a powerful microscope, it is found to be composed of many filaments, equal in thickness, continuous through its whole course, and made up each of four elementary fibres. When the nerve approaches its ultimate distribution, the filaments separate from one another, and always cross the muscular fibres at right angles; but before this separation takes place, the course is generally parallel with that of the muscular fibres. The filaments are not ultimately lost in the muscular tissue, as is commonly supposed, but either go round a certain number of the muscular fibres, and return into the twig from which they sprang, or, after crossing the muscle, enter another nervous twig running parallel to the former. In this way it appears, that, in their ultimate distribution, the nervous filaments are so many circular threads or conductors parallel to one another, and at right angles to the fibres of the muscle they supply; consequently when a stream of galvanic electricity is passed into a nerve, these threads will become the conductors of so many parallel electric currents, and therefore have an attraction towards each other. Now, if we examine, on the other hand, the microscopic appearance of a muscle in a state of contraction, we find that its ultimate fibres, originally straight, are all thrown into zig-zag lines, the angles of which are various in magnitude, but never less than 50°; and that each angle is always situated at the same precise spot, which uniformly corresponds with the point at which a nervous filament crosses the muscular fibre. The authors therefore infer, that when a muscle contracts under the application of galvanism, it is simply because a number of electric currents are sent in the same direction through the parallel nervous filaments, and so impart to them an attraction for each other, in consequence of which they approach one another, and throw the muscular fibres into zig-zag lines. All that it is necessary to assume, as the foundation of this theory, is, that the nervous tissue conducts electricity better than that of the muscles; of which, indeed, there exist pretty abundant proofs. Having thus traced the analogy which subsists between the phenomena of the electric current transmitted through ordinary conductors, and the phenomena of muscular contraction, they proceed to show, that, in all circumstances in which a muscle is thrown into contraction, electric currents are set in motion throughout its substance. This fact they establish by subjecting a muscle under contraction to the test of a very delicate galvanometer; when the contraction is excited by applying a red-hot metal to the nerve, or by touching it with nitric acid, or with the muriate of antimony, the needle of the galvanometer is turned sensibly aside. When the contractions were excited by pricking or squeezing the nerve, they could not observe that the galvanometer indicated the existence of an electric current; but they suggest, that the recent experiments of M. Becquerel on the development of the current by pressure, place it beyond a doubt that they failed, merely because the instrument was not sufficiently delicate.

We shall not endeavour to offer any opinion on the probability of these views. Their novelty and ingenuity will be admitted on all hands; but they plunge us too suddenly and deeply into the mazes of hypothesis, and are consequently by no means suited to the sober pace with which the medical world is now traversing and cultivating the waste field of physiology. At all events, the reader will take care to separate them altogether from the strict experimental deductions of Dr Edwards, contained in the prior and personal part of his work. The caution with which these deductions have been drawn must be admitted on every hand. It can only be equalled by the indefatigable care with which he has multiplied and varied all the leading experiments on which his inferences are grounded. Indeed, viewing the Treatise as a whole, we know not that Physiology can furnish a more beautiful example of strict experimental reasoning.

II.

Report from the Select Committee on the State of the Penitentiary at Milbank. Ordered, by the House of Commons, to be Printed, 8th July 1823.

In our last Number we entered into a sufficiently detailed examination of the mere medical questions regarding the sickness which appeared among the convicts of the General

Penitentiary at Milbank in the early months of 1823; and we considered, at as great length as our limits would permit, all the doubtful points to which the inquiry gave rise, under the first of those three general heads into which we divided the subject. We now proceed to examine very shortly the other two divisions, viz. the nature and justice of the charges exhibited against the late medical superintendant, and the conduct of the Managing Committee, particularly their treatment of Dr Hutchison. To enable our readers to follow distinctly the several questions as they arise, it is requisite to throw into the form of narrative various events and proceedings which either constituted these questions or gave rise to them.

II. When the time approached for opening the Penitentiary in 1816, the Committee naturally thought of providing suitable means for preserving health and preventing the inroads of sickness among its inhabitants. We learn from the evidence of Mr Holford, an intelligent member of the Committee of Management (79.), that it was at first a grave matter of consideration, whether they should have a resident medical gentleman for the convicts;—and many reasons instantly suggested the necessity of a professional person of some kind residing within the walls of the Institution. A prisoner, for example, might be taken suddenly ill during the night, and great inconvenience might result from the necessity of sending to Westminster for medical assistance;—prisoners might simulate diseases for the purpose either of obtaining extraordinary indulgence, or exemption from work; or, in short, sickness or accidents might occur in the Institution, which, without the early employment of professional assistance, might occasion serious inconvenience, alarm, or even danger, and at once cause obloquy to the Managing Committee, and great scandal to the Institution. On the other hand, they considered that they could not give a sufficient salary to induce a person of great attainments and high standing in the profession to relinquish all other employment; and it was thought expedient to have such a medical resident as they could afford,—a measure which appears to have been adopted in the appointment of Mr Pratt, previously of Kensington, to be house-surgeon to the Penitentiary. As this appointment, however, was believed inadequate to the necessities of the Penitentiary, the Committee endeavoured to supply the defect by having a consulting physician, and a consulting surgeon, who should act gratuitously (79.) Candidates for these offices were not wanting. It appears, that soon after the opening of the Penitentiary, Mr Anthony White of Westminster made application to be appointed gratuitous surgeon; on the 6th May

1816, Dr Hutchison, and, about the same time, Dr Whitter of the Westminster Hospital, and Dr Harrison of St George's Hospital, made application for the office of consulting physician on the same terms (184.) The choice of the Committee ultimately fell on Dr Hutchison and on Mr White, who were thus appointed ordinary medical advisers of the Institution. Dr Hutchison, who was consulting physician, paid great attention—much more, it is said, than was expected, to the establishment for the space of three years; and he appears, on the whole, not only to have given every satisfaction to the Committee by the manner in which he performed his duty, but to have been consulted by them on every subject in which the advice of a medical person could be either useful or necessary.

This state of affairs contined till 1819, when some changes were introduced. It appears from the evidence of Mr Thoms (184.), that in March 1819 an application was made to Lord Sidmouth for appointing Dr Hutchison medical superintendent; and we are further informed by Mr Holford, that it was intimated to the Committee in May, that Dr H. would take the medical charge of the prisoners under the care of Mr Pratt for an annual salary of 200*l.* As Lord Sidmouth replied that the act of Parliament prevented him from appointing a medical superintendent, the appointment did not then take place. Shortly after, however, when the Act of 56. George III. chap. 63. was passed, authorizing the Committee to appoint such an officer, Dr Hutchison was chosen medical superintendent to the Penitentiary, on the conditions above specified; and he, from that time, acted in this capacity.

Matters appear to have gone on with great uniformity and in perfect good order from the period of this appointment till 1822 at least. Dr Hutchison, invested with his official powers, was required to visit the Penitentiary on Tuesdays and Fridays, and, if cases required it, more frequently, or even every day; and he also made monthly and quarterly inspections and reports of the state of health among the penitents, and of all other matters relating to their management, in which medical opinion was desirable. Of the latter description were the diet—the means of air and exercise allowed the penitents—the temperature—ventilation—cleanliness, and general management of the pentagons; and the history of the Institution furnishes evidence that he had kept a watchful eye over each and all of these departments of its medical economy. It appears from his evidence of the 26th May 1823, before us, that his duties of frequent or daily visiting were confined to the infirmaries; while Mr Pratt, as resident surgeon of the Institution, had the sole care of all the convicts or penitents in the pentagons,—to see that they preserved their health,—and, if affected by disease, to send them to the infirmaries, and report to the superintendant. In the monthly inspections, however, Dr Hutchison examined all the convicts, and reported their general state of health, and other similar circumstances, to the Committee. Matters went on in this manner till May or June 1822, when the following circumstances occurred.

A young girl, named Mary Turner, about 13, certainly under 14, had been capitally convicted for stealing a quantity of valuable jewels, (303), and accordingly received sentence of death (266). This punishment, however, was commuted, under the act, into imprisonment in the Penitentiary for a certain time, 7 or 10 years. We are not informed of the date at which this young person was admitted into the prison, but it appears to have been a very short time indeed, when her health began to suffer. About the beginning of 1822, she complained of pain in the side, with slight cough, and she was admitted into the infirmary on the 7th of January. Dr Hutchison saw her a day or two after, and it appears that her complaints assumed a dangerous aspect from the first;—she was, in short, considered consumptive. As the complaints of this poor girl became daily more formidable and irresistible, notwithstanding all that skill could suggest, or nursing and attendance could perform, Dr Hutchison recommended, early in April, that it might be expedient to apply for her pardon; and to have her conveyed to the country, where she might be benefited by change of air. Inquiries were instantly made with regard to the practicability or expediency of removing her. Mr Bennet, the chaplain, corresponded with Miss Miller, a benevolent lady, the daughter of a country clergyman,—who had been much interested in the fate of the penitent; and the managing committee likewise sought for information on the same point. The eventual result was, that peculiar difficulties stood in the way of removing this girl from the Penitentiary, which, in this particular case, was not so much a place of punishment, as of shelter and accommodation. Had she been removed, there were only two places to which she could have gone, in neither of which would her situation have been so comfortable as in the Penitentiary. It would have been cruelty to have sent her to the house of her mother-in-law, (her stepmother, we presume), who was a depraved wretch, would have ill-used her, and probably hastened her death. In short, there was no place to which Mary Turner could have gone, save the Workhouse. In consequence of these difficulties, it appeared to the Committee inexpedient to recommend her for discharge from the Penitentiary; and they accordingly, on the 17th April 1822, passed a resolution to this effect—"That in regard to Mary Turner, there appearing great difficulties in the case of her removal, the Committee do not think it advisable at present to recommend her for discharge from the Penitentiary."

Meanwhile, the malady of the unfortunate child gained ground. We are not any where informed of her particular symptoms, or of their precise kind and degree; but we learn from the evidence of Mr Pratt, that on the 17th May 1822, she was reported to the Committee as "consumptive, and not likely to last long." On the 18th she is said by Mr Bennet to be dangerously ill, (266); and on the 19th, Sunday, we find she was so much reduced that Mr Pratt thought her dying; and nurse Ann Cowsey states her mortal symptoms to have been "blackness of the hands and arms, and great shortness of breathing." Dr Hutchison came to the Penitentiary in the evening between seven and eight, when this child was at the point of death, or, as Mr Pratt terms it, *in articulo mortis;* and he immediately proceeded with Mr Pratt to the room where she lay. Dr Hutchison, finding her pulse weak and quick, and her breathing difficult (215), tapped on the chest to ascertain if any fluid was effused into its cavity (267), and, with great promptitude and decision, ordered a cordial mixture consisting of camphor, ether, and aromatic confection, to be given, and a blister to be applied to the chest, (282.) It appears that Mr Pratt proposed to defer the exhibition (282) of these remedies till the other patients were seen; but Dr Hutchison instantly repeated his orders, and requested Mr Pratt to prepare the medicines that he might personally witness their administration. Mr Bennet the chaplain, whom Mary Turner expressed a wish to see, had entered the apartment almost immediately after Dr Hutchison; and it was proposed that he should administer the consolations of our holy religion to the dying person during the absence of Mr Pratt. Mr Bennet read prayers, and Dr Hutchison joined in the service in the kneeling posture (260), and he was found in this attitude when Mr Pratt returned with the draught and blister. Dr Hutchison then supported the patient to give the draught, and Mrs Ann Cowsey, the nurse of the ward, shortly after applied the blister. Dr Hutchison with Mr Pratt then left the room to visit other cases. Mr Bennet the chaplain remained; and, as Mary Turner was talking to him, she expired. This happened five or ten minutes before eight in the evening.

Such are, in short compass, the circumstances of this event, which was destined to lead to consequences of the most extraordinary character. We have neither exaggerated nor extenuated;

but have told them, as nearly as it could be collected from the evidence, that they occurred; we have narrated them also without any doubtful or contradictory circumstances, and without any of that colouring which it is easy to give stories of this kind, when it is wished to impress the imagination or interest the feelings. The unvarnished tale itself is indeed sufficiently melancholy;—and is well calculated to awaken interest in every breast not seared by long familiarity with human misery, or depraved by habitual crime. A young girl, who is described by the chaplain as well-behaved, docile and manageable, is driven, probably by the utter misery of her situation, and the contamination of the depraved wretches by whom she is surrounded, ere she had attained her fourteenth year, to commit a crime for which she is sentenced by the laws of the country to suffer death; she is, by the leniency of the Legislature, permitted to undergo the commutation of this sentence in the form of a long period of imprisonment;—attacked in the course of this expiatory confinement by a disease, which, under the best treatment and most attentive and indulgent management, is rarely prevented from destroying its victim; she bears her affliction with exemplary resignation and patience, and waits with Christian fortitude till the hand of death releases her from all her sufferings. Such an object claims no ordinary share of human sympathy, however unavailing; and the tender age, severe sufferings, resignation and early death of such a person naturally invest her history with a lively interest, which begets pain in her calamity, and a sort of long unbroken grief in the prospective certainty of her dissolution. All this we willingly admit, and should entertain a bad opinion of ourselves if we were not completely alive to the several miseries and horrors of a history so distressing as that which we have now related.

But it is not in this pathetic light that the history of Mary Turner is to be contemplated. There are at this moment many persons equally young, equally or even more innocent, and perhaps more interesting and amiable, who are drawing the last breath of a painful and tedious illness, in situations where they have neither the medical nor domestic attendance of this girl. If the heart is to be softened, and tears must be shed, we request those who are so disposed to go to such situations, and to witness such scenes. They are neither few, nor difficult to be got; and most of our readers must have more than once met with such real tragedies, and have perhaps come to the heartless and unromantic conclusion, that grief and interest and sympathy are not only unavailing and unnecessary, but it may be, injudicious. The woes of real life are indeed so numerous and so perfect in their kind, that they in a great measure extinguish much of that compassionate emotion which is natural to man, and render him, if not indifferent, at least very unromantic. Shall we, in short, speak out the truth? Shall we fix the eyes of our readers on the commonplace of deathbed sickness, and tell them to view the last moments of existence, without indulging any pathetic feeling, because many more are in the same state of mortal agony? It is perhaps both painful and imprudent to destroy illusions of this kind; but it is too true, that we meet with no instance of human misery, even in its last moments, which could not be matched, or even exceeded, by many others; and we must avert our eyes from individual instances, and contemplate the multitudes that wrestle with disease and penury and death in the workhouse, in the prison, in the hospital, in the field, in the flood, and even in the wretched hovels which poverty, whether in the freeman's rags or in the bondman's chain, can call her own. Let those whose sympathy is awakened by the picture of a single patient dying in a public institution in a great city, repair to situations of this kind. There they will find abundant scope for their philanthropy, and an incessant demand for their commiseration. Be it never so liberal,—it will soon be exhausted; let it be distributed with the most judicious economy,—it will be found utterly inadequate to the demands which are daily, hourly, and every minute even made on its generosity. In such schools they will at length see reason to think that calamity, disease, and mortal agony, are much more common and frequent than they previously imagined; but they will also learn to divest human misery of all artificial exaggeration, and to view it without sentimental refinement in its true colours; and while they are compelled to abandon their monopoly of humanity and benevolence, they will at length conclude, that all, if not vanity, is at least vexation of spirit.

We have been led to indulge in this train of reflections, by perusing the evidence relating to the history of Mary Turner; and we have put them down exactly as they rose in our minds, —completely apart from the facts of the case, that they may not make on the minds of our readers any greater or deeper impression than our readers choose. We are not indeed particularly desirous that they should make any impression whatever; and they may be passed over or read, just as the mood of the moment shall suggest. We must not, however, forget to state the reasons which, in our apprehension, have led us into this meditative digression. It is perfectly evident, we think, that the whole evidence relating to this subject is tinged with a strong dash of philanthropy and benevolence; and that sundry spots, amidst the general desert of matter-of-fact, nonsense and contradiction, are green with the verdure of humanity. The mixture, however, is incongruous; and the effect, instead of proving to be of that tranquillizing and refreshing quality which it is said to be in the natural world, is ludicrous and disgusting in the extreme. Mr Bennet is the only person who is at all tolerable, or speaks like a person in his senses; yet even he informs us, that he felt hurt, because Dr Hutchison turned pale, and said, "is she?" when the death of Mary Turner was announced. The others indulge in a sort of mawkish, maudlin benevolence and gentleness, which reminds us of nothing so much as a wretched debauchee half drunk,—protesting, with tears in his eyes, against the horrible vice of intoxication, and deploring the iniquity and corruption of the age. Mr Pratt, for example, is very much agitated at one time, and actually sheds tears at another; and is exceedingly shocked and astonished at the prescriptions of Dr Hutchison. Mrs Ann Cowsey, the nurse, 'was much attached to Mary Turner, was quite shocked at the rough manner in which Dr Hutchison patted her, and ran out of the room crying very much,' and protesting "she could not stay in the room any longer, while Dr Hutchison was pulling that poor child about." 275. Mrs Wilkinson, the matron, 'thought it was a pity to disturb her, when she was so near death;' and even the very convicts are so tender-hearted, as to be astonished and shocked at the daring cruelty of the medical superintendent. In short, the demon of humanity had taken possession of every person in the neighbourhood of Mr Pratt on this memorable evening, except the medical superintendant himself, who appears to have been utterly obdurate and insensible to every appeal of this description.

Now, we by no means doubt that these same tender-hearted persons really felt all the compassionate emotions which they describe: and it may be, that they were actually distressed with the sight of the last sufferings of Mary Turner. But it is not only unreasonable, but quite preposterous, to suppose that others did not feel for the situation of the poor girl; and it argues the most daring and conceited presumption to convert their individual feelings and emotions into a standard for those of all other persons. What proof is there that Dr Hutchison was not actuated by the most lively and genuine humanity for his patient? None certainly in his want of attention or care during the course of her illness. He had prescribed, at an early period of her disease, such remedies as the rules of art suggested. When these were ineffectual, he recommended a measure which is justly believed to be more efficacious than all medicine and all medical attention, and without which medicine rarely avails. That the execution of this measure was impracticable or inexpedient, was no fault of Dr Hutchison, or even of any one concerned, but resulted solely from the peculiar circumstances of the case. But in what manner was Dr Hutchison to manifest his humanity and attention to a patient who was rapidly sinking under incurable pulmonary disease? Was it by abandoning the patient passively and absolutely to her fate, without making a single effort either to ascertain the probable state of the diseased organs, to subdue morbid action, or to alleviate urgent and painful symptoms? We are almost certain there is scarcely an individual in the profession, unless Mr Pratt courts this ignominious distinction, who will venture to avow doctrines so absurd. It is justly remarked by Sir Gilbert Blane, that none of us, however experienced, or however sagacious, can tell with certainty, that any given fit of difficult breathing and fainting, is to be the last, even in consumption; and it argues the most criminal and ignorant dogmatism, to refuse the exhibition of any remedies calculated to give relief, as long as life continues to languish in the breast of the sufferer.

Our reflections, and the events which gave rise to them, have led us to anticipate somewhat the progress of our narrative. What now remains is very short, and shall be quickly told. Dr Hutchison reported the death of Mary Turner, in the course of business, to the Committee. Mr Pratt, who was already possessed with the demon of false philanthropy, became restless and uneasy in his mind; he went about like a person labouring under an evil conscience or a disturbed imagination; and perhaps with the view of quieting his own, he laudably resolved to begin disturbing those of other people. The first object of this man's machinations was the chaplain, Mr Bennet, to whom he presented himself some two or three mornings after the death of Mary Turner. He then apologized for his intrusion, and stated that he was not satisfied with the conduct of Dr Hutchison, who, according to his account, had been in the habit of coming to the Penitentiary of an evening in a state of intoxication. With consummate hypocrisy, and a complete mockery of humility and deference, he stated that he came to consult Mr Bennet in what way he should prefer a charge against Dr Hutchison, and at the same time mentioned Sunday evening particularly, and endeavoured to make Mr Bennet assent to his having observed Dr Hutchison in this state on the occasion of Mary Turner's last moments. Mr Bennet, however, not only denied that he observed any thing of the kind, but positively asserted that nothing led him to suppose Dr Hutchison under the influence of wine; and Mr Bennet finally refused to give any opinion on the subject.

Mr Pratt, however, was not thus to be conjured to a state of quietism. After the failure of his attempts on Mr Bennet, he resolved forthwith to select one of the visitors, and he proceeded to instill his poison into the ear of Mr Courtenay. It is stated by this gentleman in his evidence, that some time in the month of June, Mr Pratt requested permission to speak with Mr Courtenay in private, on a matter which he represented to be of great importance to the Institution, and the source of much difficulty and embarrassment to himself. Having premised his communication in this mysterious and consequential manner, he proceeded to acquaint Mr Courtenay, who waited on Mr Pratt at his house, in the Penitentiary, that he wished to mention some circumstances which had occurred on the occasion of the death of Mary Turner, and which circumstances had placed him (Mr Pratt) in a very *awkward and responsible situation.* He then related the circumstances attending the last moments of Mary Turner, the visit of Dr Hutchison, and the prescriptions, and stated particularly that Dr Hutchison, after remarking that she was *in articulo mortis*, had ordered her the warm bath, and concluded, by adding his impression that Dr Hutchison was then under the influence of wine. Mr Courtenay having asked whether this had been observed on any previous occasion, Mr Pratt replied that the medical superintendant frequently appeared to be hasty and impatient at his evening visits; that he would not undertake to say whether such haste or impatience arose from the effect of wine or not; but he stated it as the result of his opinion, on the particular occasion of Sunday, 19th May, that the conduct of Dr Hutchison arose from excess of wine. Mr Courtenay having learnt the names of the individuals who were present, examined the matron and nurse apart from Mr Pratt, but with as much delicacy as a report so improbable and malicious evidently required. The matron confirmed the statement of the facts delivered by Mr Pratt; and appeared to be under the impression, that Dr Hutchison was affected by wine. The nurse manifested a reluctance to state any thing prejudicial to Dr Hutchison; but Mr Courtenay asserts that he collected from her an impression to the same purpose.

Mr Courtenay, under these circumstances, conceived it to be his duty to call the attention of the Committee to the subject; and at the next ordinary meeting, after requesting the Secretary to leave the room, he made a report of the incident, and its alleged attendant circumstances. The Committee evidently desired to proceed with circumspection and delicacy; and with this view, excluding their Secretary as has been already stated, they delegated to Mr E. Bootle Wilbraham, one of their number, the task of writing to Dr Hutchison a letter, which was intended at once to be most friendly and confidential, but at the same time sufficiently authoritative and official. It is there stated, that the Committee "had discovered with regret and concern, that a suspicion is entertained by different individuals, and is even spreading among the prisoners, that the directions which you give in cases which come under consideration after dinner, are marked with a haste and precipitation which are not observable in an earlier part of the day. Without being more explicit, the Committee think it right that you should be acquainted with the fact; they are unwilling to believe the suspicion well-founded, and conceive that any formal inquiry might produce consequences injurious to your well-founded reputation; they beg you to be assured that their motive is most friendly, dissuade you from taking any steps to disprove the grounds of the suspicion, it being obvious that the matter never can be brought to issue, and recommend you to trust to time to dispel the impression;" and Mr Wilbraham concludes, by requesting "an acknowledgment of this letter in writing."

Now, it is obvious that this letter was the very way to kindle the most complete conflagration that ever raged among the members of an incorporated body—the very blast by which the whole of the lukewarm embers of discontent, jealousy, and every other inflammatory ingredient were at once to be blown into an irresistible and destructive explosion. The letter first rouses the curiosity of the medical superintendent, by hinting some horrible suspicion—some unheard of piece of delinquency; but furnishes him ample scope for conjecture, by declining to be more explicit. It then professes an unwillingness to believe this malicious suspicion; but deprecates any inquiry, as if it was certain to be confirmed. And, lastly, admitting that it never can be brought to issue, strongly recommends, however, to trust to time, and good conduct we presume, to efface the impression, as if it were quite established that the suspicion or impression, whatever it was, was well-founded. We have rarely seen such singular inconsistences—such gross deviations from the liberal principles of English justice. We always understood that it was a standing rule to presume every man innocent till he is distinctly proved to be guilty; we should imagine that *a fortiori* the justice of this principle in public life, in transactions involving no crime, must be unequivocally recognised and made the rule of conduct. Yet here is the reputation of a respectable physician hinted away, with the same deliberate coolness with which Domitian would impale a fly, or an old dowager whisper away the honour of a fashionable beauty; and all because an incendiary of an apothecary chooses to tell a groundless and incredible story, probably of his own fabrication.

But whatever effect the letter of Mr Wilbraham was calculated to produce, we know very well what actually did follow. Dr Hutchison replied and reclaimed in a tone both of anger and indignation—related as a vindication the occurrences libelled—appealed to the gentlemen of the Committee whom he had often met in the evenings, if they ever recognised any incorrectness, or any proof of his being less fit for duty than at other times—and demanded, as might have been foreseen, inquiry into the report, and the persons concerned in its fabrication. To this Mr Wilbraham rejoined, in the name of the Committee, on the 13th June—expressed the regret of the Committee that Dr Hutchison was not disposed to take the well meant and friendly advice formerly offered, especially as subsequent circumstances proved more strongly the necessity of the communication then made—assured him that the information on which they were acting could not have originated in malicious motives, and was comfirmed by more than one or two persons—strongly suggested the expediency of reconsidering his demand for inquiry—and concluded with a hope that the superintendant would enable Mr Wilbraham to lay before the Committee, at their next meeting, a letter *more in unison with the view of the subject which influenced them in the communication he was directed to make, than that which he had the honour of receiving.*

A long reply from Dr Hutchison, on the 14th June, contains certainly a more temperate examination of both letters, but expresses substantially the same opinion on the conduct of the Committee, and the advice which they tendered. He also deprecates, in the strongest language, and, at the same time, with arguments which to us appear satisfactory and convincing, the possibility of a medical person, who is often engaged in the evening in various professional or scientific duties, indulging in such a vicious habit, and yet retaining the confidence and esteem of his friends and professional brethren. Enclosed in this communication were the strongest testimonials of sobriety, and even abstemiousness on many occasions, from Sir Gilbert Blane, Dr Granville, Dr Baird, Mr John Finlayson, actuary of the national debt, and Mr R. W. Bampfield. To these persons Dr Hutchison had been known as a friend and as a medical adviser for a series of years; and in no instance did any thing ever occur to change or diminish the opinion they entertained of the character of Dr Hutchison. This answer and the testimonials were laid before the Committee, who examined the matron and nurses on the incidents of the evening of the 19th May; and the result of this examination was such, that Mr Courtenay and Mr Wilbraham were desired to commune with Dr Granville as the friend of Dr Hutchison, and state the circumstances of the case for his judgment. The statement of Mr Wilbraham sets forth, that this measure was taken to prevent Dr Hutchison the disgrace and inconvenience of a public charge, such as he demanded. We have endeavoured to recount merely the facts of the proceedings, without adverting at all to motives, which are always difficult to be learnt, and must, in many instances, completely elude observation. For this reason we say nothing of their justice at present, or of the reason with which Dr Hutchison could dread any public examination; but we shall show subsequently, when considering the evidence adduced about the affair of the bath, what degree of credit may be given to the testimony of the persons to whom the story is traced.

The effect of the interview with Dr Granville is curious. On the afternoon of Sunday the 16th June, Dr Granville having learned from Dr Hutchison his regret that the Committee had not selected two, instead of one of his friends, and his resolution to allow nothing short of the names of the individuals who had made the charges, to induce him to forego public investigation, with the approbation of Messrs Courtenay and Wilbraham sought Sir Gilbert Blane, another warm and judicious friend of Dr Hutchison.

"To him he inclosed the communications made by the two members of the Committee, and they (Dr Granville and Sir Gilbert Blane) together visited Mr Hutchison on the same evening; Dr Granville then informed Mr Hutchison, that he was instructed by Mr Bootle Wilbraham and Mr Courtenay, to assure Mr Hutchison that *the Committee never believed the charge of ebriety made against him to have any foundation;* that they hoped, for the good of the Institution, he would not press the public inquiry into the said reports against him; that they would satisfy Mr Hutchison as to the individuals from whom those reports emanated, and who were all present on the particular night specified in the charge; and that they would depend on him for not taking any public notice of their communication, or not so resenting it as to interfere with the duties of the officers in the prison; that they then named to Dr Granville, *the chaplain, the surgeon, the matron, and a nurse of the name of Cowsey.* Dr Granville and Sir Gilbert Blane then urged in the most strenuous manner upon Mr Hutchison the propriety of acceding to this proposal made by the Committee; that it was a concession from a body of men that ought to satisfy the most scrupulous feelings of injured reputation, even had the charge been made public, but particularly as the whole transaction had been so delicately conducted by the Committee." p. 223.

In consequence of this measure, the following letter was transmitted by Dr Granville, from Dr Hutchison to Mr Wilbraham, and was received, as this gentleman says, as Dr Hutchison could have wished the Committee to do.

"Dear Sir, *Leicester Square*, 17*th June* 1822.

"The communication conveyed to me last night by Dr Granville, from the deputation of the Committee, has not only relieved my mind from an infinite degree of anxiety, but has impressed me with sentiments of the highest gratitude to yourself and the other members of the Committee concerned in the late most painful discussion, for your very friendly and most kind demeanour towards me.

"By whatever misconception of your letters, my friends and I happened to receive the impression that I had actually fallen in the estimation of the Committee, still, while such an impression existed, it was natural that I should strenuously exert myself to recover their good opinion; and, believing myself under the intolerable imputation of a habit foreign to my nature, it will, I hope, be a proof that I value their good opinion the more highly, that I could not bear the apparent loss of it on such grounds with tranquillity.

"It being explained, however, by Dr Granville, that the Committee never entertained the idea that I had conceived to have influenced them, I can have no hesitation to receive the wish conveyed in your first letter, as gratefully as I have no doubt it was meant kindly; and I beg you will be pleased to convey to the Committee my respectful assurance, that no effort on my part shall ever be omitted to impress on all concerned in the Penitentiary, a conviction that I am incapable, at any hour of the day or night, of failing in my duty, and it will require very little additional circumspection on my part to convince them effectually, both by my manner and practice, that I have in reality no other motive than the strictest performance of my professional duties; I should indeed deeply lament that any circumstance could for a moment induce them to think otherwise. I have the honour to be, &c. A. Copland Hutchison."

"E. Bootle Wilbraham, Esq.
&c. &c. &c."

The business was thus brought to a favourable termination by a course which ought to have been pursued from the beginning,—by assuring the superintendant unequivocally of the Committee's disbelief in the charge, and by naming the persons on whose testimony it was uttered, and acted upon. There can be little doubt, that, considering the source from which it issued, it ought to have been stifled in its birth; or,—if was deemed expedient to give it any attention whatever, that the Committee should at once have come forward frankly and stated to Dr H. what they heard, and the grounds on which the report was propagated. Such conduct would have saved much chafing of blood, and much wounded feeling, and would have shown the respective parties the confidence mutually reposed in each other. The course which was ultimately adopted by the Committee, on the contrary, though it produced a reconciliation, only postponed the final rupture to a more distant period. The peace which was thus patched up was hollow and temporary. The fires of distrust, rancour and jealous discontent still smouldered in the breasts of both parties, and were ready to break forth under the most trivial excitation. This opportunity was destined not to be long wanting.

Our readers know, that in the beginning of 1823, the sickness commenced and extended in the Penitentiary; the Select Committee of the House of Commons was appointed to inquire into the state of the Institution; the inquiry commenced, and all the written documents and correspondence relating to the affair of Mary Turner's death, which had been sealed up and deposited in the hands of the secretary, were now opened and submitted to examination. Witnesses were also examined, and depositions were taken from visitors, from physicians and surgeons, from chaplain, from house-surgeon, from matron and from nurses. The whole matter, indeed, was canvassed and investigated with as much attention and zeal as possible, and nothing appears to have been omitted, which might determine the justice of the charges originally brought against Dr Hutchison in June 1822. Yet, with all this indefatigable inquiry, it is astonishing how little is established, and to what a small number the competent witnesses are reduced. We have been occasionally compelled to anticipate some of the results of this evidence in our detail of transactions. We now proceed to advert to the evidence itself, and we trust that we shall not appear to have drawn a single inference, however trifling, which does not prove to be most strictly legitimate.

The charges advanced against the medical superintendent are not very specific, if we except the single one of intoxication on the evening of the 19th May. The others of roughness, as they call it, or harshness, and inconsistency in ordering and counter-ordering, when divested of the unnatural bulk into which they are magnified by the witnesses, are most completely vague, frivolous, and ridiculously insignificant. We conceive that they are so unimportant, that they may be completely relinquished for the specific charge, and that it is quite unnecessary to bestow any other than incidental attention on them in the course of examining the other. Now, it is curious that the whole of the specific charge is quite *inferential*, or a *deduc-*

tion in the minds of the witnesses from facts, which they observed, or say they observed. It is not a charge depending on a direct fact of observation; but it is their mode of explaining facts which, to their comprehension, were the source of astonishment. How far these respectable persons had reason for astonishment, will, perhaps, appear in the sequel. In other words, the specific charge of intoxication is involved in an order for a warm bath, which is said to have been given on the evening of the 19th May.

We have already recorded the circumstances of the last moments of Mary Turner, and mentioned in general terms the visit, conduct, and prescriptions of Dr Hutchison. It is said, he prescribed at the same time with the cordial mixture and blister, immersion in the warm bath. This report, for it cannot in its present state be regarded in any other light, has given rise to two questions; 1*st*, Was the bath actually prescribed? 2*d*, Was this remedy to a patient in the last stage of consumption, or said to be moribund, expedient or improper, right or wrong?

1. On the first of these points,—whether the bath was actually prescribed, the Committee have made numerous inquiries; but it cannot be said that these inquiries have contributed to settle the question in either way; and, certainly, they have not positively proved that it was prescribed. It may be laid down as an established principle, that the only way in which information regarding such a question can be obtained, is by collecting the parole evidence of those who were present at the time the prescription was issued; and though, in this manner, it must be admitted, there is ample room for discrepancy and contradiction, we find that the whole may be reduced to very slender and insufficient sources. Of a prescription issued, as those of the Penitentiary appear to have been, it is scarcely possible for more individuals to have been conscious, than the superintendent and surgeon—the prescriber and the person to whom the prescription is immediately addressed. The physician does not, as in a clinical ward, or in a public hospital, pronounce his prescription aloud, until it is recorded by a clerk, or audibly heard by a number of pupils; nor is any regular record kept, to which, in case of mistake, neglect or other misapprehension, an appeal might be made for the confidence and satisfaction of all concerned. The prescription is, on the contrary, uttered in a colloquial manner by the one to the other, who forthwith, or at his convenience, takes steps to have it put in execution, (134, 135.) With these circumstances borne in mind, let us see by what means the question of the actual prescription is established.

There is some degree of difficulty in ascertaining what persons were present on the evening of the 19th May,—at the moment at which the prescription was issued; for no two witnesses give the same answer to the question. Mr Pratt states, that besides the medical superintendant and (the surgeon) himself, there were only Mrs Ann Cowsey, the nurse, and three prisoners, Jane Beaumont, Rosina Pennington, and Sarah Mitchell (259.) According to Mrs Ann Cowsey, on the other hand, there were present, besides the superintendent, surgeon, and herself, five prisoners or women, Jane Beaumont, Elizabeth Collison, Mary Ann Butcher, Sarah Mitchell, and Rosina Pennington, (268.) It appears not quite certain, whether Mr Bennet the chaplain was present at the precise moment at which the prescription was given; for Mr Pratt only says, "I think he was," (259); whereas, nurse Cowsey, when interrogated, directly, "Was the bath ordered before Mr Bennet came in, or afterwards?" answers with equal confidence, "Before Mr Bennet came in; because the matron and Mr Bennet went into my room during the time that Dr Hutchison prescribed," (209.) Mr Bennet himself states, that he was in the room at the time Mary Turner died, and that he met Dr Hutchison in the nurse's room. To avoid cavil, therefore, we shall admit, in the meantime, that he was present at the moment alluded to. Mrs Elizabeth Wilkinson the matron, also states that she was present; and that a bath and blister, and some medicine were ordered in her presence, (274.) Yet it is remarkable that neither Mr Pratt nor Mrs Cowsey, when positively interrogated, and naming the individuals who were present, mentions Mrs Wilkinson among the number; and it is only after the question is put in a particular form, that Mr Pratt admits the chaplain and matron to have been present.

"Do you recollect, that in addition to those you have named as being in the ward with Mary Turner on the evening of the 19th, that there was the chaplain, the matron, and the nurse Cowsey?—I do recollect that they were in the ward." p. 263.

The only individuals, in short, who are admitted by all to have been present at the moment of prescription, besides Dr Hutchison and Mr Pratt, were the nurse Ann Cowsey, and the three prisoners, Jane Beaumont, Rosina Pennington, and Sarah Mitchell; and the direct evidence should be therefore restricted to that of Dr Hutchison, Mr Pratt, Mrs Cowsey, and these prisoners. All other must be regarded on this point as indirect and hearsay.

Dr Hutchison himself most positively denies that he either ordered or could order a bath for Mary Turner.

"Upon this point, I can only repeat, that however the nurses, or Mr Bennet, may have understood my prescription, both my reason and my recollection suggest to me the impossibility of having ordered a bath for the child. I remember perfectly well ordering a bath for some one, and that this order was attempted to be evaded by a frivolous objection of there being no steam to warm the boiler, which excuse I put down at once, by ordering the kettles to be placed on the fire to prepare warm water; but as I should not certainly myself have pursued such a line of practice in the case of Mary Turner, I never can attribute the idea of the bath being ordered for her, to any other cause than some inexplicable confusion, which at this distance of time it is impossible to account for." p. 283. *Letter of Dr H. to the Select Committee.*

This acknowledgment shows that a bath was ordered, but not for Mary Turner.

The testimony of Mr Pratt is confident and positive.

" Who were present?—Three or four of the prisoners and Mrs Cowsey the nurse. Dr Hutchison examined her, and he advised me immediately to get a blister and a cardiac mixture, and æther and a blister to apply to the stomach. At the same time Dr Hutchison desired a warm bath to be prepared for the purpose of putting her in.

" Putting her in?—Yes, putting her in.

" You are sure it was Mary Turner he meant?—Yes, certain of it." p. 257.

" You have stated there was no person in the infirmary to whom the bath would be a proper application?—Certainly not.

" Have you a list of the persons ill in the infirmary at that time? —I have.

" Is there an account of all the diseases?—Yes, there is.

[*The same was handed in.*]

" Was there any person in the infirmary that day ill of chronic rheumatism?—Yes, there was.

" What was her name?—Ann Shorter.

" Is she in that list?—Yes, she is.

" Will you take it upon yourself to say the bath was not ordered for that woman?—I will.

" Of that you are sure?—Yes." p. 259.

" Can you tell me, whether Mary Turner had ever been in a warm bath before?—I do not recollect that. (260.)

" You are quite certain this bath was not ordered for any other person whatever?—That I am certain of.

" Do you consider, as a professional man, a warm bath a good thing for chronic rheumatism?—Certainly, I do.

" Then did you ask Dr Hutchison, if the warm bath was meant for Mary Turner?—Dr Hutchison and I had not seen the chronic rheumatism case that night, we only saw Mary Turner when it was ordered.

" Do you believe that Dr Hutchison did not know of the case of chronic rheumatism in that ward?—It was not in that ward, but in another part of the infirmary.

" Was this woman suddenly seized with it?—A chronic rheumatism is what has existed for some time.

" Why are you sure this bath was not intended for the chronic case?—He had not seen that case that evening.

" You still doubt whether Dr Hutchison might not have known of that patient?—He knew it from his previous visits." p. 260.

" Ann Shorter, who was the rheumatic patient, was on Mrs Barnacle's side?—She was.

" And not where this woman was?—No.

" Had Dr Hutchison at that time seen Ann Shorter?—He had not.

" Did he give directions about the warm bath to Mrs Cowsey or Mrs Barnacle?—I believe to Mrs Cowsey." p. 261.

" Were there any other patients in the room?—Four patients and the nurse.

" It could not be to any one of these patients that the bath could apply?—No.

" Was the bath given to any body?—No.

" Was the bath prepared?—It was not; Dr Hutchison forbad it afterwards.

" The kettles were put on?—I believe they were.

" Do you know that of your own knowledge?—No." p. 262.

" Did you not state to Mr Courtenay, that Dr Hutchison ordered the bath for Mary Turner after he had said she was *in articulo mortis?*—No; I told him that was my observation.

" That was previous to his examining her?—Yes; upon his going into the room.

" You were present when the bath was ordered?—I just heard the words given for the bath being ordered.

" There were no words expressing who it was for?—Except for Mary Turner.

" Did you hear any such words?—I took him to that patient immediately; upon which he gave an order for the medicine and the warm bath.

" He afterwards, you say, gave an order that the bath should not be administered?—He did." p. 262.

" Do you mean to say there was no person in that ward for whom a bath might be considered necessary?—Certainly not.

" Upon what do you found that judgment?—Upon the number of patients in the room.

" What were the diseases under which they were labouring?—There was Elizabeth Collison, and Mary Turner, and Sarah Mitchell, I believe that was all.

" Was not there a fourth patient in the ward?—I believe not.

" Is it not a very common circumstance, that a person admitted into the infirmary with one disease, before being removed, being attacked with another?—That has certainly occurred.

" Frequently?—Yes.

" May not it have been possible, although there may be no disease against the name of the individual here in that ward for which a bath might be necessary, that such might have been the case in this instance?—Certainly it was not.

" Upon what do you give so decided an opinion?—Upon the ground of the cases that were there; Elizabeth Collison was consumptive, Mary Turner was consumptive, Sarah Mitchell was an inflammation of the bowels.

" Had she not an inflammation of the liver?—Not to my knowledge.

" Did she not become dropsical?—She did.

" Is it usual to prescribe a warm bath for a person labouring under chronic or acute inflammation of the liver, or diseases of the abdomen?—Certainly it is.

" Is there not a possibility that the bath may have been ordered for this woman?—Certainly it was not.

" Not a possibility of it?—Not in my mind; I can positively say the bath was ordered for Mary Turner.

" Were not you in the act of going out of the ward when the order was given for the warm bath?—I might be just at the door at the time the order was given.

" Should you say positively, that before your going out of the door that the bath was ordered for that individual patient?—I will say so; that the bath was ordered for Mary Turner.

" You have said, that on going out of the door you heard the words " warm bath" accidentally?—I heard the order given for it being prepared.

" But you did not hear the name of the person?—Dr Hutchison's attention was called to that person.

" You have said, you heard the words " warm bath" just as you were going out of the door?—Yes, as I was going to the door.

" Was it an usual thing for Dr Hutchison to go into the ward to see a particular patient, and not ask the others how they were?—Dr Hutchison's attention was called to that particular patient." pp. 263, 264.

" Will you inform the Committee, why in your journal of the circumstances of that case that evening, it does not appear that the bath was inserted in the journal; but only the medicine prescribed for that girl?—Certainly it was not inserted in the day-book, but the medicine prescribed was; nor has, I believe, the word " bath" been inserted at any time, because the order was given upon the spot where the bath could be procured." p. 246.

The testimony of Mr Bennet may be understood from the following extracts.

" You are chaplain to the Penitentiary?—Yes.

" Do you recollect the death of Mary Turner?—I do.

" Were you in the room at the time she died?—I was.

" Were you there before Dr Hutchison and Mr Pratt came in, or did you come in afterwards?—When I was sent for to Mary Turner in the evening, Dr Hutchison was in the nurse's room, we both met together; I believe he was in the room at the time.

" Did you make any observation to Dr Hutchison, or Dr Hutchison to you?—Mrs Cowsey, one of the nurses, was there; and she said she was very ill, and he obliged her to be put into a bath.

" Was any thing said by Mr Pratt to Dr Hutchison, or by Dr Hutchison to Mr Pratt, about her being *in articulo mortis?*—I do not recollect.

" Did you hear Dr Hutchison desire she should be put into a bath?—I did; when he ordered her to be put into the bath, and the answer was from the nurse, the steam was off, and they could not get the water.

" Did you make any observation upon that at the time?—No; Dr Hutchison desired the nurses to put on kettles; I then said, if that is the case she may come out of the bath exhausted, I will go and read to her first, which I did, and Dr Hutchison followed me into the room.

" Where was Dr Hutchison at the time the order was given?—In Mrs Cowsey's room.

" Did you go into the room where the sick woman was?—I believe Dr Hutchison followed me immediately; for when I got up from reading prayers, Dr Hutchison was in the room, and I think, to the best of my recollection, he went in with me when I had read prayers. Dr Hutchison countermanded the bath, and ordered a blister and some medicine; the blister was cut in two; he said she was not to go into the bath." p. 265.

We have already adverted to the uncertainty of Mr Bennet having been present; the following questions should be noticed for the information they communicate on the exact locality in which the parties relatively were.

" If the Committee understand you, the order was given in Mrs Cowsey's room?—Yes.

" Had he seen the patient?—I think he had.

" Was Mr Pratt there?—Yes.

" Did you hear Dr Hutchison order the bath in the sick room?—It was in the nurse's room, adjoining.

" You say, the bath and blister and the medicine were ordered together?—No, certainly not; that I cannot answer, for I only heard the bath ordered at the first; and I remember the nurse brought a large blister, and a small one, which was cut; that was after the bath was countermanded." p. 267.

Now, this testimony, with that which is subsequently given by nurse Cowsey, and the matron Mrs Wilkinson, presents the most contradictory and inconsistent statements that could be imagined. Not one of them agrees with the other in regard to the place at which the bath was ordered, which they must certainly have done, had the order been given at all, or had it been given in the case of Mary Turner. Mr Pratt, for example, says, that Dr Hutchison was in the room with Mary Turner when he ordered the bath; while Mr Bennet asserts that he was in Mrs Cowsey's room, (265, 267.) Mrs Ann Cowsey, on the other hand, asserts that the bath was ordered "before Mr Bennet came in, because the matron and Mr Bennet went into my room during the time that Dr Hutchison prescribed," (269.) Lastly, Mrs Wilkinson the matron states, that she and Mr Bennet were present in the room in which the patient was, when a bath, a blister and some medicine were ordered,—which is equally inconsistent with the other evidence.

Yet glaring as this discordance must appear in the eyes of all who know how to estimate the force of parole evidence, it is not even the only or the most material instance, in which they are not unanimous. If the individuals who were said, on that memorable occasion, to have been present, were actually present, we think it is consonant to observation, that they must have remarked the condition of the patient with at least the same attention with which they remarked the prescription. It might reasonably be expected, that any two of them who were examined on the state of Mary Turner, at the moment the bath was said to be ordered, would give nearly the same account of her last moments. Yet this scene, which must have been, and evidently was impressive, is described in very different terms by Mr Pratt and by Mr Bennet,—the only two witnesses who were interrogated on the subject. Mr Pratt contends in his whole evidence, that Mary Turner was insensible and could not speak or articulate;—Mr Bennet, on the other hand, states that she was not insensible; and that, instead of being unable to speak or articulate, she preserved her faculties and speech to the very last, and expired when in the act of talking to him. We request the reader to contrast the evidence of these two witnesses.

"To Mr Pratt. If the patient was so far gone as to be insensible, where was the use of reading prayers?—I do not know. *Could she speak?—Very little, barely articulating;* she was dead in ten minutes afterwards.

"To Mr Bennet. She was not in a state of insensibility when you read prayers?—Certainly not; I was with her when she died, and she was perfectly sensible, and spoke shortly before. *She was perfectly sensible?—Perfectly so.* Did she speak in a low tone of voice?—*She spoke plain;* she appeared quite exhausted; she died in a moment when talking to me. Dr Hutchison said she might live a short time." p. 266.

Such may be said to be the evidence, direct and collateral, to prove that a bath was ordered for Mary Turner; and we appeal to every unprejudiced person to know whether any positive conclusion could be confidently drawn from it. Is it not evident that these witnesses have contradicted each other on the most material points of the inquiry; and have they not shown, what might have been expected,—that not one of them knew to whom the bath was ordered, if it really was mentioned at all? It is indeed difficult to conceive, if it was ordered in the usual manner in which such prescriptions appear to have been given, how it could be heard by so many persons. That Mr Pratt may have heard the order, must be admitted; Mrs Cowsey also, the nurse, might hear it; though it is most probable that she must have done so through the channel of Mr Pratt, and not directly from Dr Hutchison; but that it should be heard directly by Mrs Wilkinson, Mr Bennet, and four or five prisoners, is a very unlikely circumstance indeed. After the most careful comparison, the evidence concerning the prescription of the bath must be reduced to one of two alternatives. *1st*, It may have been that the order was given in the ward, when Mr Pratt, according to his own account, was going to the door, and when he heard it, or the words "warm bath," without its being applied to any person individually. In this case it seems doubtful that the chaplain could have heard it directly, and it is certain that Mrs Wilkinson could not have heard it, if she was, as is stated, in Mrs Cowsey's room. *2d*, If the bath was ordered, as the chaplain states, in Mrs Cowsey's room, how were the prisoners to hear it, and to express astonishment at the moment, as is represented by the nurse and matron? Either case presents inconsistencies which cannot be reconciled on the statements of any of the witnesses.

2. On the second point, whether the bath was an expedient or proper measure to a patient in the circumstances of Mary Turner, it is very difficult to give a decided and absolute opinion. In the treatment of a disease like consumption, very opposite plans and remedies have been recommended and tried; and it not unfrequently happens, that what is reputed an efficacious remedy at one time, becomes reprobated in the strongest terms at another. The tepid bath, at 94° Fahrenheit, has been recommended by Dr Armstrong, who states, that he has found it useful in alleviating the heat and general irritation of hectic fever, and rendering the pulse slower, the breathing freer, and diminishing pain or weight in the breast.* Other

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* Practical Illustrations of Scarlet Fever, Measles, Pulmonary Consumption and Chronic Diseases, by John Armstrong, M. D. p. 508.

practitioners, however, are so hostile to this remedy, that they would not only disapprove of it, but employ a remedy which certainly is, in many of its effects, diametrically opposite;—we allude to the tonic and cold sponging system. So unsettled are the principles and practice of physic. It must be admitted, that the use of this remedy, even on the authority of those who recommend it, is not suited to the earlier periods of the disease; and therefore, its approved employment in such circumstances is little applicable to the subject at present under consideration. The question now to be determined is, the expediency of the warm or tepid bath to a patient believed to be moribund, in the last stage of consumption. Sir Gilbert Blane and Dr Granville decidedly disapprove of it as a remedy on general grounds entirely; but Sir Gilbert admits, that there is much diversity of opinion on this subject, and quotes an instance in the practice of Dr Jebb, thirty or forty years ago, in which he proposed it in a case not unlike; and Dr Granville, though he admits it to be bad practice, yet grants, that its propriety in the particular case alluded to, must be determined by the contingent circumstances. We do not regard it possible to determine a question of this nature by proofs derived from *a priori* arguments. We do not perceive on what principle the measure can either be praised or condemned in a disease, the treatment of which is either very empirical in the hands of the most eminent physicians, or, at least, is conducted according to the peculiar views and observations at the time of the respective attendant. But, we are farther of opinion, that none but a very dogmatic or a very ignorant person, would express himself on this point in the strong language which Mr Pratt has used before the Select Committee. Our limits will not permit us to bestow any farther attention on the precious matter which this person has communicated, and we must request our readers to examine his evidence for themselves, p. 258–264.

The secret of the specific or grand charge now comes out. The medical superintendent examines his patient and prescribes, —not to leave her quiet, as Mr Pratt would have done, nor to let her breathe out her last convulsive gasps, unaided, and unfelt for; but a cordial mixture, and a blister,—and, what most astonished the surgeon and his friends, a bath. Why prescribe the bath? What reason could induce Dr Hutchison to order a bath for one who is dying? they ask. Unreasonable waste of hot water! Very obvious, they reply, after a little reflection. A degree of irritation, says Mr Pratt;—too much wine, says Elizabeth Collison;—inconsistency, and a kind of irritation, responds Mrs Wilkinson. 'Tis certainly irritability of manner and a little excess! returns the shrewd and observant Mr Pratt;—shocking cruelty! re-echoes Mrs Cowsey;—too much liquor, concludes Mr Pratt; and so it was settled that the medical superintendent had tarried too long at the wine cup. We cannot adduce any thing like this,—any instance in which a charge of intoxication was so easily and rapidly made out; and yet this may be said to be the whole of the grounds on which Mr Courtenay, Mr Wilbraham, and the other gentlemen of the June Committee, 1822, so readily proceeded to criminate the Superintendent. The whole charge indeed was a mere inference which Mr Pratt and his sapient friends chose to draw, from the circumstance of a warm-bath being ordered, when they thought it should not have been ordered, and countermanded, by their report, when means to prepare it were wanting. We have great pleasure in remarking, that however puzzled Mr Bennet was to account for this warm-bath, he did not disgrace his reasoning powers by joining the inference of Mr Pratt. "No," says this gentleman to Mr Pratt, "I did not observe that Dr Hutchison was intoxicated; nothing led me to suppose that Dr Hutchison came in that state. Dr Hutchison displayed some irritability of temper; he seemed dejected and out of spirits. I had no reason to think this dejection arose from such a cause. I believe Dr Hutchison hardly ever comes to the Penitentiary of an evening without calling on me, and *I have never seen him even in a state approaching to intoxication.*" (266.)

We see, therefore, from this statement, which is substantially correct, that the Visitors' Committee fell into a small mistake in adding the name of the Chaplain to that list of wretches, who gave it as their opinion that Dr Hutchison was under the influence of wine. It is indeed remarkable, that when the direct question is kept in view, this charge is restricted to a small number of witnesses indeed. Elizabeth Collison, one of the prisoners, appears to have remarked, that "Dr Hutchison would not have done so, if he had not had too much wine;" and this, with Mr Pratt's own assertions, forms the whole amount of the evidence on the subject.

But if the charge appears unfounded, on inquiring into the evidence on which it rests, it is completely annihilated by the testimony of several respectable individuals examined by the Select Committee. Testimony of a general nature is given by Edward Goulburn, Esq., Dr Roderick M'Leod, Robert Milligan Dalzell, Esq., Mr Robert Wade, Mr Thomas Walker, Sir Astley Cooper, and Admiral Lord Gambier, to show that the habits of Dr Hutchison are not merely temperate, but on some occasions abstemious; and, to satisfy the most strict and scrupulous inquiry, we have the testimony of Mr Frederick

Fownes, and Mr James Tweedale, as to the capacity of Dr Hutchison on the evening of the 19th May 1822. It appears from the evidence of the former gentleman, that Dr Hutchison was at the same time giving his professional attendance to the family of the brother-in-law of Mr Fownes, and visited a child at nine o'clock that evening, when he exhibited every appearance of the most perfect sobriety. Mr Tweedale is a surgeon, had known Dr Hutchison since 1811, and was accustomed to consult him professionally. In 1822, Mr Tweedale was called to a case of fractured thigh-bone, which took place in the person of an infirm old woman on the 18th May. Dr Hutchison's opinion was requested; and he visited this patient on the 19th,—the very evening on which his manner at the Penitentiary was canvassed by Mr Pratt and friends.

"What time was it?—In the evening about seven o'clock; we had a candle, she kept a little grocer's shop, and it was rather dark, we had a candle, it was in a back room.

"Was it nearer six than seven?—I think it was; it was before I had taken tea, I took tea after I returned home; it was in Long Acre.

"Are you certain it was the 19th?—It was the 19th, I am quite certain; because I had been looking at the day-book before I came out.

"You are certain it was after dinner?—Yes; I am quite certain of it.

"Was Dr Hutchison then in a state of sobriety?—Perfectly.

"Did you see any thing in his manner, that led you to think he had been drinking wine?—Nothing at all.

"Did he smell of wine?—Not at all.

"Do you smell well yourself?—Yes; perfectly.

"You did not find out he smelt of wine that night?—I did not.

"You say, between six and seven o'clock on the 19th, he attended a patient under your care, who had fractured her thigh?—Yes; the neck of the thigh bone, and it is not well at present; I was not satisfied with my own opinion and I called in Dr Hutchison; I would also state, that about a month before that, in the evening, I called upon Dr Hutchison to tie up an artery.

"Was that in the evening?—Yes.

"Was he perfectly sober?—Yes.

"Did he smell of wine?—No; not all." pp. 280, 281.

If any thing could be conclusive and to the point, this certainly must be admitted to be so. It is so satisfactory as to require no commentary whatever; and we leave it to our readers, with the persuasion, that whatever was the cause of the "irritability" on which Mr Pratt dwells so strongly, his ascribing it to intoxication could originate either in profound folly, or profound knavery, or matchless assurance.

III. The third general question presents several subjects, the consideration of which is extremely delicate, rather unpleasant; and, upon the whole, what most persons will regard as exceedingly humiliating and discreditable to all concerned. We are perfectly aware of the extreme difficulty of acquitting the medical Superintendent, without criminating the managing Committee,—or at least imputing to them motives and conduct which we would wish to disclaim. With every wish also to believe the Committee actuated by no other feeling save zeal for the welfare of the Penitentiary, and for the success of the great national experiment which was thus tried to diminish delinquency, and check the incipient growth of crime, we find it exceedingly difficult to explain their conduct to Dr Hutchison. The feelings of hesitation to which this difficulty gives rise, with the knowledge of the circumstances above related, and of those which are yet to fall under notice, are any thing but a source of gratification; and it is sometimes our wish, either that they had not occurred, or at least that it had not fallen to our lot to notice their existence. We must not, however, indulge in anticipating reflections, but proceed to state the facts as they took place, and let them give birth to remarks in the proper place.

We formerly took occasion to mention the period at which sickness began to prevail among the Penitentiary convicts, and at which the Infirmaries of the Institution began to be filled with patients. The letters of the Superintendent, of the 14th, 24th, and 28th February, announce the increase of cases reputed scorbutic; in that of the 24th, the number of cases of flux had greatly increased; and in the 28th, when four cases are reported, two are said to be attended with flux. This increase of disease appears to have alarmed the Committee; for we find that they held on the same day a special meeting, for the purpose of considering what should be done, and came to the following resolution.

"That two physicians be professionally employed to visit the infirmaries; to communicate with Dr Hutchison and Mr Pratt, and and to give such directions for the treatment of the patients as they may think proper; and that they be requested to direct their attention to the causes of such sickness, and particularly to inquire how far the same is to be ascribed to the dietary. That for this purpose they be desired to meet either daily, or so often in the week as may appear to be necessary, and for such period as may enable them to form a decided opinion upon the subjects referred to them." (364.)

In pursuance of this resolution, a copy of which was transmitted in the most polite manner to Dr Hutchison, the Committee, on the same day, addressed a request to Dr Maton and Dr Latham, that they would undertake the duty of visiting the Institution, and examining the condition of health among its inhabitants. Dr Maton having declined the task thus imposed on him, Dr Roget was substituted in his place at the next ordinary meeting, 1st March; and, on the same day, Dr Latham and Dr Roget visited the Penitentiary, and commenced their duties of survey and examination. The subsequent Report is well known, and was certainly calculated to diminish the confidence which the Committee previously reposed in Dr Hutchison; and we find, indeed, one of their number expressing his opinions to this effect.

> " The committee of the Penitentiary certainly heard with much surprise, after they had only been told by Doctor Hutchison in the course of the month of February of the existence of a few cases of scurvy within their walls, that 400 cases of that kind were discovered in the investigation which took place in the beginning of March; but they did not complain of Doctor Hutchison for having left them so entirely in the dark respecting the state of the prison previous to the appointment of the two physicians; on the contrary, Doctor Hutchison complained of the appointment by the committee, of the two physicians to act with him in the prison; and represented this proceeding as unnecessary, and as a great indignity to him." p. 117.

Now, it is quite undeniable that the Committee had reason to be both alarmed and dissatisfied; and it was sufficiently reasonable to adopt the measure of requesting additional assistance, especially where the interests of a great public Institution were at stake, and where so much of its ultimate success, and its general popularity as a system of penal restraint, depended on proper and unobjectionable management in the beginning. But it must, on the other hand, be kept in mind, that Dr Hutchison was not so much to be blamed, as, under other circumstances, he must have been. He has distinctly proved in his evidence that his duties, as medical superintendant, were of two kinds, constant and periodical;—that his constant duties were confined to visiting the patients in the infirmaries, whereas the periodical duties consisted in monthly inspections of the penitents in the pentagons. He has shown that it was, properly speaking, the duty of the surgeon Mr Pratt to attend to the state of the penitents in the establishment at large, to watch over their health during the intervals between these monthly inspections, and to report to him in the event of sickness, injury, or other needful occasion. Now, the time at which the Committee passed the above resolution for additional assistance, was exactly upon the eve of Dr Hutchison's monthly visit; and indeed, Drs Latham and Roget visited and inspected on the same day on which Dr Hutchison was to have done so in the regular course of his duty, (Postscript to Letter, 2d May, p. 348); so that this resolution had the effect of depriving him of almost the only merit which he could have.

It would be unjust and unreasonable to imagine that Dr Hutchison should not have inspected the persons of the convicts, and remarked their degree of health and strength; and it would be supposing him less acquainted, than he really is, with those professional duties, in the exercise of which much of his life has been spent, to say that he should not have recognised, as easily as the other physicians, those marks of disease, on which they laid so much stress in their Report. Whether he should have subsequently ascertained what the formidable disease actually was,—and in what part the most powerful and obstinate cause of sickness principally rested, we cannot pretend to offer an opinion. We have in our former article shown, by proofs and arguments which we conceive to be strong and almost unanswerable, that the malady which was most common and most fatal among the convicts, was inflammation of the mucous membrane of the intestines; and there is proof that he did not overlook this circumstance entirely. Dr Hutchison, in his letter of the 24th February, remarked, that the number of cases of intestinal disease (flux) had greatly increased, and that one person, Humphry Adams, had died with these symptoms,—and states his opinion that this might be scorbutic dysentery; and in his letter of the 28th, after announcing four more cases of scurvy, two of them attended with intestinal affection, he is led to advert to the influence which the prison-diet might have in begetting the disease; and distinctly states, that " he is now quite confident that it is not sufficient to shield them against attacks of scurvy and scorbutic dysentery." In the same letter he suggests means of cure and prevention, which, on this view, appear to be rational and efficacious.

In these reports, therefore, to the Managing Committee, we can discover neither remissness, indifference, nor ignorance. The rise and progress of sickliness among the convicts were accurately marked and faithfully reported, in so far at least as a judgment could be formed at the time; and there was no reason to presume that Dr Hutchison could, on the 1st of March, have been less active or sagacious than the gentlemen then summoned, in recognising the phenomena and prevalence of bad health among the inmates of the Penitentiary. Thus far, we think, we may advance with certainty in asserting, that

Dr Hutchison was not chargeable either with remissness or careless observation. After this period we have no report on the health of the penitents, unless the quarterly one from him on the 8th April, in which he states scorbutic dysentery and diarrhœa to have been the chief source of mortality. (365.) We have therefore no means of discovering, how far he might be correct in any views, which he might subsequently take of the disease. The report of the physicians stated it to be scurvy, and diarrhœa or dysentery of a scorbutic character: and all inquiry was thus superseded and rendered gratuitous, by the strong and unequivocal terms in which this opinion was expressed. The conclusion which flows from this is, that the observation and diagnostic powers of the medical superintendent were at least not inferior to those of the physicians-extraordinary. We however proceed with our narrative.

We have admitted the propriety of the Committee requesting extraordinary and additional assistance on occasion of the sickliness spreading rapidly among the convicts; and for the same reason we admit, that it became Dr Hutchison to acquiesce in the prudence of this measure, and to do every thing which might render it beneficial, or in any way contribute to its successful issue. It is our painful duty, however, to remark, that this friendly cooperation, if it ever existed, did not last. We will not pretend to assign reasons for this, lest we should misrepresent. It is sufficient that we notice the simple fact. On the 1st of March, when Dr Hutchison went to the Penitentiary, he found a note from Mr Courtenay at the porter's lodge, stating that Drs Roget and Latham would be there at two o'clock. It was already two, and they were there. Dr Hutchison states in his evidence that he communicated with the extraordinary physicians in the freest and most unreserved manner (51, 52); and Dr Roget gives the same account of their communication and cooperation in his evidence (23). The first proof of any deviation from the best understanding appears, so far as we can discern, in the evidence of Dr Hutchison (45), when assigning a reason for the occurrence of diarrhœa as a consequence of a change from salt provisions to fresh meat and vegetables, in persons affected with scurvy. To the question of the Select Committee, whether he communicated this observation to the Committee of Management, Dr Hutchison answers in the negative. Why not, said the Committee. It had no reference, replied Dr Hutchison, to the disease then existing; all my duties were superseded at the time the disease was discovered; and, when farther questioned, he states, that since the appointment of Drs Latham and Roget, the Committee had uniformly sent for those gentlemen and communicated with them, and not with Dr Hutchison.

There is strong reason to believe, that, in forming this opinion of supersession, Dr Hutchison acted at least spontaneously, if not prematurely; for Mr Holford expressly states in his evidence, that the appointment of the extraordinary physicians was not intended to supersede his attendance.

"Not only were no instructions given, nor any hints thrown out to induce them to slight him; but I desire to say affirmatively, that there was the greatest desire, on the part of the Committee, that Dr Hutchison should be treated with that degree of attention which his standing in the profession, and the relation in which he stood towards the Committee, demanded." p. 119.

Both Mr Holford and Mr Courtenay also state, that the object of the Committee in requesting extraordinary medical advice, was precisely the same as would be in a private family, and that the same cooperation and the same harmony of consultation was expected to be observed. But whatever was the intention, and however well they appear to have managed at first, no long time elapsed ere circumstances occurred which indicated that this tranquil mode of proceeding was at an end. Three different statements, at least, are given by Mr Holford, Mr Courtenay, and Dr Hutchison. The two first vary very little,—that of Dr Hutchison differs in certain points, which, a person in his situation, would undoubtedly regard as very material. One of the first causes of difference evidently arose from the prescription and prohibition of lemon-juice, which Dr Hutchison regarded as the best and only remedy, and which Drs Latham and Roget considered improper, or, in short, unnecessary. When Dr Hutchison, therefore, ordered lemon-juice, he was told by Mr Pratt that it could not be given without asking permission of the extraordinary physicians; in other words, that his orders could not be complied with. Circumstances of this kind appear first to have shown Dr Hutchison the unpleasant and inefficient situation in which he was placed; and even Mr Holford is obliged to admit this inference.

"Do you not think, if a communication was made to the superintending physician by another medical officer in the establishment, that his orders would not be complied with, that it must be considered by him as superseding him in the execution of his duty?—I certainly do think so; and I conceive the effect of that ought to be to make him distinctly apply to the Committee, or resign. I had not the latter part of the alternative in my contemplation when I gave that answer to Mr Pratt; I expected that, if Dr Hutchison persisted in wishing to adopt a different treatment from that which the other two physicians were pursuing, he would have applied to the Committee, which he would have had an opportunity of doing in a day or two afterwards. I felt myself in a very awkward situation with respect to Dr Hutchison; at the same time, I could not tell the surgeon that he must obey two masters, and must give the lemon-juice that Dr Hutchison prescribed, when the other physicians might be adopting a different course of treatment. Any conversation of this kind I had with Mr Pratt was as a private member of the Committee; I gave no order upon the subject, but only advised him for the best." p. 119.

But, according to the evidence of Dr Hutchison, it was not this circumstance alone which made him regard his authority as annihilated. A minute statement of the awkward and ill-sorted arrangement which thus existed at the Penitentiary, is brought out in the inquiry into the case of Ann Martin, who laboured under disease of the spine, and, consequently, was considered under surgical treatment. Drs Roget and Latham had not been desired, it is said, to consult with Dr Hutchison in this case, as it was of that nature in which they did not interfere. Ann Martin was not taking lemon-juice, and therefore, the prohibition could not apply. But they were giving her brandy, when she wished to have wine; and when Dr Hutchison requested this wish to be gratified, Mr Pratt assigned, as his reason for not doing so, that Mr White was attending, and it could not be done without his permission. Thus, from one slight difference, they appear to have proceeded to another, till the breach was gradually widened, and at length rendered irreparable. The following questions and answers give a tolerable view of the relation in which the several parties stood to each other.

"Do you mean to say, that for a month previously to your leaving the Penitentiary, you had been in the habit of writing prescriptions that were not attended to?—Yes, I had been in the habit of prescribing for individuals, that the surgeon would not make up the medicines for.

"What could induce you to continue drawing up prescriptions, which from experience you knew were not attended to?—It was my anxious wish to ascertain what was the cause of all those objections on the part of the surgeon. I wished to ascertain whether it was of his own accord, or whether from any special directions he had received from the Committee, or any particular member of the Committee, or from the physicians, or consulting surgeon, or from whom the thing emanated." p. 134.

"Did you consider yourself justified in going round and prescribing for the prisoners singly, after Dr Latham, Dr Roget, and Mr White, had been professionally called in?—The fact is, that as the principal medical superintendent of that Institution, I do not think that my services there ought to have been superseded in the way they were. I went there frequently; I tried to fix a period with those gentlemen to meet; and on one occasion I said to one of them, ‘ I wish you would fix a period to-morrow.’ ‘ No, I cannot, indeed, it would be very inconvenient.’ I replied, ‘ my inconvenience would be as great as yours.’ When a period was fixed, they did not come so regularly to the period as I did; and my time was as valuable to me as their's to them. I went therefore round the wards; and if those gentlemen had found that the medicines prescribed were not such as met their approbation, it was for them then to make any alteration they chose.” p. 135.

Next came the letter of the 8th April, in which Dr Hutchison expressed his understanding that the two extraordinary physicians were required to *act* and *report independently of him;* and in the postscript of which, he announced his intention of sending a copy of it to the Secretary of State for the Home Department, because Mr Peel had consulted him on the health of the prisoners. To this Mr Wilbraham, in the name of the Committee, replied on the 12th,—that in consequence of “ the evident indisposition on your part to hold any useful communication on subjects connected with your medical duty, with the superior officers of this establishment, it appears impossible that your attendance on the prison can go on with comfort to us or yourself, or with advantage to the public;” and that “ they had, therefore, felt it their duty to summon a meeting to take into consideration the removal of Dr Hutchison, and the appointment of another physician as his successor,” 366. They likewise gave a hint at the expediency of resignation. Dr Hutchison rejoined first on the 14th, and again on the 19th. In both, he vindicates and remonstrates; contends that he never manifested any disinclination to communicate with any of the officers. of the Institution, and declines accepting the suggestion of the Committee. The resolution of removal, accordingly, was entered on the minutes on the 19th April, and Dr Hutchison ceased to be superintendent.

Such is the history of these transactions from their origin to their termination. We have endeavoured to state them in a manner as free from any bias either to the one side or the other, as it is possible to do in a matter of this nature; and we have refrained from expressing any opinion either on the one side or on the other. We do not know that Dr Hutchison has been altogether very well used, or treated in the most liberal manner in the world. But if we except the injudicious proceedings about the transactions of the 19th May 1822, it is not easy to say in what particular circumstances the unmerited treatment consisted, or from what source it proceeded. There is no proof, in our opinion, of any thing like conspiracy, either among the visitors or among the officers of the Institution,—though this has been stated in pretty broad terms by more than one witness, and is occasionally adverted to by the questions of the Select Committee. Mr Morton Pitt, indeed, a venerable and respected member of the Managing Committee, expresses his distinct belief that jealousies and cabals existed among the officers of the Institution from its commencement; that these were either encouraged, or not sufficiently discouraged, through mistaken notions of their effects; and that Dr Hutchison was the victim of a system of tale-bearing and intrigue. It would be foreign to our purpose to inquire into the grounds of this opinion; and we have indeed no inclination to plunge into all the petty and complicated domestic transactions and postscenial interests of a national prison; but we may be permitted to say, that, though there are abundant proofs of injudicious conduct, and of too implicit confidence being reposed in the statements of the domestic servants of the Institution, we can discover none of a regular system of conspiracy against the superintendent. It is evident that Mr Pratt indulged in a very perfect hatred of the superintendent; but such a person would have hated any other superintendent,—merely because he knew him to be a more sensible and accomplished physician, and because he was obliged to keep this fresh in his little soul by the daily remembrance of the authority to which it was his duty to submit. The conspiracy of such a person must have been contemptible indeed; and the very character and talents of this arch-apothecary prove that no such plot could be formidable to Dr Hutchison.

On the subject of the removal of Dr Hutchison, it would be impertinent to offer any opinion. We have related the circumstances which led to this measure; and we could not discover, in the proceedings of the Managing Committee, any thing to which objection could be reasonably made. To every body of men who preside over a public institution, full and uncontrolled authority in the choice of their officers and servants must be given. They are responsible for every thing done within the precincts of the establishment; and the legislative body, from whom their authority is derived, naturally look to them when any extraordinary or unexpected inconvenience occurs. It would be therefore quite unreasonable to restrain them, or exercise any authority over them, in the selection of those persons by whom the several requisite duties were to be performed. On the propriety of these principles, which we think will be denied by no reasonable person who is acquainted with the mode in which the affairs of public institutions are conducted, we justified the managers in their measure of requesting the advice of extraordinary physicians. That this measure was succeeded by transactions which indicated either disrespect to the medical superintendent, or a studied wish to render his situation unpleasant, and at length insufferable, was certainly not contemplated by the Managing Committee; but they nevertheless do not appear to have made any provision to guard against such consequences. The whole business of the extraordinary medical assistance seems to have been conducted, not only without regard to the feelings of the superintendent, but by allowing the house-surgeon to continue his system of misprision or clandestine malversation, with a degree of inattention which was next to exclusion from office. Notwithstanding all these sources of chagrin, we know not how far it was consistent with the situation of the superintendent to appeal to the authority of the Secretary of State for Home Affairs. To say the least of this proceeding, it was obviously furnishing the Committee with a pretext which they had not hitherto possessed. Whether the removal was inconsistent with any conditions specified, it is impossible to determine without knowing the terms of the original contract;—whether the office was *ad vitam et culpam*, or merely *durante bene placito.* It is obvious that the measure of the Committee, which was immediate and imperative, was dictated under the latter impression.

On the conduct of the superintendent we must say very little. His situation was delicate, and the part which he had to perform was difficult. But we should feel more for him if he had remonstrated less, and done more; if he had suffered the Committee of Management to go on in their own way, without that incitement to arbitrary and authoritative measures, which uniformly results from remonstrance and angry reclamation. His letters, at the same time, breathe a spirit of petty vexation, which, we think, it was imprudent to display. It would have shown more spirit and more dignity to take no notice of the various plots and counter-plots, which he says existed in the management of the Penitentiary; and to have treated the whole conduct of every individual about the Institution, from the Chairman of the Committee, down to the notable Mr Pratt, and the amiable Mrs Cowsey, as greatly too unimportant to give him any uneasiness, while he was conscious of performing his duty. The conduct of all such persons, whether high or low, must have been nothing to him, while he discharged his duty with punctuality, and conducted himself in other respects with firmness, independence, and dignity; and if their conduct continued to be improper, the same principles should lead him at once to renounce every engagement with them. The moment he discovered that they were listening to the tales of inferior servants, he should have resigned his office, and taught the managers that no man of liberal feelings would continue to act with instruments so vile. There is no liberal profession in which the reputation of its members lies so much at the mercy of the most unprincipled of mankind, as that of medicine; but this necessary evil should have no other effect than that of showing the necessity of independent and dignified conduct, which will rarely fail to be the best defiance of all cabals and machinations.

The principal error, into which we conceive the managers had fallen, was that of listening too much to the representations of one set of their servants to the detriment of others. In the case of Dr Hutchison, it is singular to see men of their extensive knowledge and liberal minds, lend an ear to the stories of such persons as Mr Pratt and nurse Cowsey. We do not know that we could produce an instance of any gentleman, whose knowledge, strong good sense, just views on all subjects, and especially on those connected with the Penitentiary, are more conspicuous than Mr Holford. The extensive and accurate information which this gentleman possesses on every topic relating to the management of the Institution, and the zeal and attention with which he has watched over its interests, can only be understood by careful perusal of his evidence, and of the masterly and instructive reports to which his name is attached in the Appendix. Of the other gentlemen we cannot speak so particularly; but the little that appears of them is sufficiently creditable. Yet how these eminent persons, or a committee of which Mr Holford was a member, could listen with implicit confidence to Mr Pratt's miserable tales, is a complete anomaly in the history of Corporation economy. One honourable exception we find in the person of Mr Morton Pitt, who not only discouraged this system, but all along disapproved of the proceedings against Dr Hutchison. It is indeed difficult, without entering into the justice of removing the superintendent, for any person who is guided by candour, and that sense of the propriety of respecting a confidential medical officer, which is at least prudent if not necessary—to approve of the whole proceedings of the Committee of Management. It is not understood that a public body of men should be hasty or precipitate in their measures; nor is it decorous to reverse or repeal these measures, unless on the force of weighty and satisfactory reasons. They had not made choice of Dr Hutchison as their medical superintendent in a rash or hasty manner, and with insufficient knowledge of his character, manners and professional qualifications. He was fully three years in their service as mere consulting physician, bound to the Institution by no tie but that of professional duty, ere he was invested with the office of medical superintendent. During this period the Managing Committee must have had ample opportunity of estimating the fitness of Dr Hutchison to be their servant; and when they did appoint him, therefore, it could not be said that they had done so without knowledge or trial of his capacity. After appointing a medical officer such as Dr Hutchison, they should have reposed every confidence in him; and this should not have been withdrawn till flagrant proofs of his unworthiness appeared. This, we conceive, would have been at once more just to him, and more creditable to themselves, than the ambiguous and hollow *friendliness*, with which all their covert discontent was veiled. In the most lenient view, it was an indignity to rest the character of any individual fit for the office of medical superintendent on the suspicion and absurd inferences of persons who actually did not know their own minds.

We have, however, dwelt much longer than enough, on a subject by no means agreeable. The changes in the affairs of the Penitentiary have not yet terminated. We have just learnt that Drs Roget and Latham have been *removed* from their office. We cannot conjecture the cause. Mr Pratt has been at length dismissed with a pension of 100*l.* a year—as a reward for all his good deeds; and to close the whole proceedings properly, Mr Anthony White, formerly consulting surgeon, has been appointed medical superintendent with a salary of 300*l.* No compensation or remuneration has been given to Dr Hutchison.

III.

Elémens d'Anatomie Générale, ou Description de tous les Genres d'Organes qui composent le Corps Humain. Par P. A. BECLARD, D'Angers, Professeur d'Anatomie à la Faculté de Medécine de Paris. A Paris, chez Bechet Jeune, Libraire de l'Academie Royale de Medécine, Place de l'Ecole de Medécine, N°. 4. 1823. 8vo. pp. 728.

AN elementary work on the structure, anatomical characters, and physical properties of the several tissues of the animal body, has long been wanted; and we think that the present is well calculated to supply the deficiency. Bichat's work on General Anatomy applied to physiology and medicine, was beginning to be somewhat behind the state of knowledge and the progress of anatomical research; and its descriptive details of organization were perhaps too much blended with physiological principles and pathological conclusions not always established on the most certain foundation. It was, moreover, too long and too voluminous for ordinary readers. In a book on the organization of the elementary tissues, simple and untheoretical arrangement, accurate enumeration of physical and physiological properties, description of organization or minute structure, derived from careful and repeated observation, in the most condensed and abridged form possible, are the chief virtues; and the book which possesses these qualities in the greatest degree, will undoubtedly be the most useful and valuable. In this respect the single volume of M. Beclard has unquestionably the advantage of the two bulky tomes of Bichat; and though it is the only work in the field on the same subject, except the first volume of the unfinished Anatomy of Dr Gordon, it may be said to be the most deserving of the attention of the student, and by no means likely, for some time at least, to be supplanted by any other competitor.

Our readers will probably remember, that in 1821, M. Beclard published additions to the General Anatomy of Bichat, and that we gave rather a minute account of this publication in our 71st Number. * We then alluded very shortly to the circumstances which rendered such a commentary requisite; and we gave such a sketch of the state of knowledge as our limits permitted. We may now remark, that the same reasons which then induced M. Beclard to publish his additions, have operated with greater force in requiring the publication of the present work. It is, by the author's acknowledgment, a mere summary of the anatomical course which he has been in the habit of delivering for a period of ten years; and, though completely independent and perfect in itself, it is the first or introductory part of a general treatise of human anatomy. M. Beclard further acknowledges, that though he has been guided in its preparation by the work of Bichat, he has taken care to add all the most valuable information of subsequent authors; and we find that he has not only availed himself with advantage of the researches of Moreschi, Meckel, and others, but has also improved several of its departments by the correct observations of Dr Gordon. The work, indeed, may be regarded as somewhat like Bichat's system, stripped of its speculative digressions and problematical theories, and reduced into a much narrower compass by a more rigid adherence to mere descriptive details,—but adapted to the increased knowledge and corrected views of the present day, by the addition of every thing valuable, from the researches either of former or recent anatomists. Another characteristic of the present work, not always found in those of France, is the scrupulous justice which is done to the individual researches of foreign anato-

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* Vol. XVIII. p. 247.

mists,—German, Italian, and English;—and a copious and exact list of references to the writings of those authors who have most distinguished themselves in the investigation of the individual subjects. In this respect, M. Beclard appears to much greater advantage than his predecessor, not only in his more correct acquaintance with the literary history of his science, but in his rigid observance of historical justice to those anatomists, whose claims Bichat has been accused of overlooking.

M. Beclard has commenced his work with an introduction or preliminary dissertation, consisting of two sections; in the first of which he treats of the properties and characters of organized bodies, of animals in general, and their divisions into invertebrated animals, vertebrated animals, and of oviparous and viviparous or mammiferous vertebrated animals; in the second of the human body, of its fluids, of its organs, of organism, of the development and differences of organization, of alterations of organization, of death, and of the lifeless body. In the consideration of the subjects of the first section, the author acknowledges his obligations to the researches of MM. Dumeril, Blainville, Geoffroy St Hilaire, Lamarck, but especially Cuvier; and to all who know how much this eminent anatomist has done in elucidating the philosophy of animal bodies, the avowal that his name must have been quoted in every page, will not appear extraordinary or incredible. In the second part, on the human body in general, Reil, Blumenbach, Ackermann, Mehlis, Plenck, Chaussier, Hebenstreit, Hubner, and Lawrence, are the chief authorities. The views are, in general, correct and elaborate;—to expect them to be original is unreasonable, and, we should almost say, impossible. They are sufficiently minute for the purpose for which they are intended; as correct as they could be in such limited space; and the references will enable those who have leisure and inclination to pursue the subject in the works of individual writers, as far as may be expedient or desirable. We do not pretend to enter into any thing like a minute analysis; as we have often in our periodical labours given such views of this subject as our readers would be inclined to expect from us; and we may refer them to our article on the "Additions" of this author for such observations on the nature and objects of general anatomy as are suited for our pages. It may not, however, be unreasonable to glance very shortly at some of the subjects treated by our author in his Preliminary Dissertation. It is good for our professional readers to be gently drawn from the details of mere practice to the philosophical generalities of the physiological sciences; and an occasional glance at the connection which subsists between organized beings of the most opposite and unlike characters, will prevent them from converting their anatomical studies into a mere enumeration of bones, muscles, nerves and arteries. We are, therefore, not unwilling to take this opportunity of refreshing their ideas on the various forms of organized life. To these reasons we may add, that it is convenient to exhibit a condensed and abbreviated sketch of the result of the observations and reflections of Cuvier and Lamark, by one so likely to do them justice as M. Beclard.

Besides the rounded figure which belongs in general to all organized beings, most animals are externally at least symmetrical, and divided by a middle, or vertical line, into two similar halves, and their length in the direction of this line is generally more considerable than the other dimensions. The body contains one internal cavity, which receives the alimentary matters, and which is covered by a membrane or internal skin, continuous with the skin of the outer surface. Many animals have circulating vessels, through which the nutritious matter of the intestinal cavity is conveyed to all parts of the body;—respiratory organs in which this matter is submitted to the action of the atmosphere;—and secretory organs in which a part is separated from the mass. They have genital organs, which generally consist in a cavity from which the germs issue. Lastly, most animals have muscles to perform certain motions, senses to receive the impressions of external objects, and a nervous system consisting of lengthened cords, and round concentrated masses. After adverting to the proportion of the animal solids or organs, and the animal fluids or humours, the author proceeds to give general views of the nature of the general organic phenomena, nutrition and generation, and the particular phenomena of animal life, or those of muscular motion, sensation, and nervous power or action. These animal functions, when they exist, exert a considerable influence over those which are merely organic. In that of nutrition, the introduction and transmission of food is accomplished by muscular organs; in that of circulation, muscles in many animals are found at the origin of the vessels; and, lastly, the application of the atmospherical air to the organ of respiration depends on the action of muscles. Organs of sensation are situate at the entrance of the organs of nutrition; and the acts of generation, like those of nutrition, are modified by the animal functions. These general expressions of a group of facts show, that a definite relation subsists between the several functions, which either exist in the animal body, or distinguish it from other living beings.

To these general characters of animal bodies, the particular ones derived from figure, from organ, and from function, and the distinctions thus suggested, succeed. These we shall take the liberty of omitting, not because they are uninstructive or uninteresting, but because our readers are perhaps more disposed to study them in the work itself; or in the preliminary dissertations of Cuvier, from which, principally, they are very correctly compiled. We shall, however, lay before them the enumeration of the animal classes and orders adopted by M. Beclard.

After noticing the striking distinction which is established among the classes and orders of the animal kingdom, by the absence or presence of a vertebral column, M. Beclard proceeds to enumerate the animals destitute of vertebræ in the following order.

I. The animals destitute of vertebræ (ANIMALIA INVERTEBRATA) contain three great divisions, the Radiated (*Animalia Radiata*), the Jointed (*A. Articulata, Annulosa*), and the Molluscous (*A. Mollusca.*)

A. The Radiated animals constitute a particular class, of which the essential character is,—that the parts are disposed in the manner of rays round a central point. Their structure, which is simple, presents nevertheless varieties, from the *hydræ* or armed polypi to the *asteriæ*. All of them inhabit the water.

1. The polypi, a numerous class, are generally elongated, with a single aperture or mouth provided with radiated appendices; they have an alimentary cavity, digest very rapidly, and absorb by imbibition. Their outer and inner surfaces are similar; in the intermediate substance, which is of a homogeneous and gelatinous form, nothing can be distinguished but microscopic globules. They are so regenerative, that if divided, each part becomes an individual. Light, sound, and other external agents, produce in them impressions followed by motions. Some are fixed to the spot, others are free. The most simple are the *hydræ*, which have an alimentary sac, and multiply by germs or buds. Others, which are united, excrete from their outer surface a hard or calcareous matter called Coral (*polypier*). * Lastly, in some which are compound animals, the common body envelopes a secreted substance, the consistence of which varies from that of jelly to stone.

2. Of the *acalephæ* (ακαληφαι) or *sea-nettles*, the shape is most distinctly circular or radiated, and the structure varies. The mouth is in the centre, is furnished with *tentacula*, and leads to a stomach which in many is ramified, but which has no other aperture or outlet.

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* This term has been introduced into the nomenclature of Natural History by Lamarck, as a general name for the excretions, whatever be their consistence, of that order of animals so long named Zoophytes.—*Histoire Naturelle des Animaux Sansvertebres*, Tome II. p. 13.

3. The *echinodermata*, or sea-urchins, are radiated animals, asteroid, spheroid, or cylindrical, with more complex organization. An internal cavity, with distinct organs; vascular productions from the intestine ramified through the body; and distinct outlet from the intestine; aqueducts ramified for the purpose of respiration; ovariform masses for the purpose of generation; muscles, and in most of them numerous *tentacula* terminating in suckers, generally termed feet, are the principal characters of this class. The skin is well organized, and often solid; and in some we meet with nervous filaments.

B. In the Articulated or Jointed animals, the body is symmetrical, divided externally into a certain number of rings or segments movable on each other, and formed by skin more or less firm, sometimes hard, except in the intermediate spaces where this covering is soft and flexible. Their muscles are attached inside the skin; their nerves are cords enlarged at intervals, and situated beneath the intestinal canal. Organization of the most varied character is found in this class.

1. Some are vermiform, without head, and articulated feet, and limited to the motion of creeping. These are the *worms proper*, and the *annelides.*

a, In the intestinal worms (*helminthi*), which have some relation to the radiated animals, the body is oblong, cylindrical or flattened, bare, soft; no organ of respiration, or of circulation; and the generation is internal gemmiparous, and sexual, oviparous. They inhabit the bodies of other animals. Their degree of organization varies. In the tape-worms (*cestoides*), which are the simplest, and resemble a long striped ribbon, marked with a longitudinal line, no external organ, nor even suckers, are perceived,—and internally nothing but egg-shaped atoms.

Others (*trematodes et tenioides*), of which the shapes are varied, have merely suckers more or less numerous, ramified in some through the body, which also presents other gemmiferous or ovariferous canals. The *acanthocephali* (*echinorhinci*), have a tube provided with hooks moveable by muscles, two small intestines without outlet, and distinct oviducts, or spermatic receptacles, according to the sex, which is separate. Of the *nematoides*, or cavitary worms, as the ascarides, the organization is still more complex. A mouth, an intestinal canal, with proper outlet, skin provided with muscular fibres, generally marked with cross streaks; distinct genital organs, with marks of separate sex, a dorsal and ventral nervous chord, and two blood-vessels, are the characters of this tribe.

b, In the *annelides* or red-blooded worms, the oblong body is divided into numerous rings, the first of which, forming the head, is different from the others; and the mouth is either a tube or jaws. An intestine traversing the body; a double system of arteries and veins without distinct hearts, red blood and branchial respiration; hermaphrodite genital organs; muscles, and in many hairy or silky productions (*soies*), which are used for feet; a head with tentacula, and in some black points, which are taken for eyes,—are the chief characters of this class.

2. Of the other articulated animals, all are provided with a head, eyes simple or compound, a mouth, consisting either of lateral jaws or a sucking tube, and complex and varied organs of digestion. Their chest sustains six jointed paws, each joint of which is hollow and contains the muscles which move the next one; their skin is crusted and firm; they have the sense of smell, but its seat is not determined; and their generation is sexual and oviparous. This section consists of three great classes, insects proper, *arachnides*, and the crustaceous animals.

a. The insects proper, or six-footed (*Hexapoda*) animals, must be well known to all. A vestige of heart, in the form of a dorsal vessel, to which no branches have been perceived, contains a white fluid, which appears to be conveyed through the body by imbibition. Respiration is performed by branching *tracheæ*, which meet in two principal trunks. The organs of motion are muscles and skin, hardened by a horny matter contained in its substance, six articulated paws, four wings in most, two in some, in a few none. The organs of sensation are compound eyes, in some smooth, ordinarily to the amount of three, *antennæ*, and feelers. They smell and hear, but the organs of these sensations are unknown. It is well known that these animals copulate once only in the course of their life; that the impregnated female deposites its eggs in a convenient spot; and that the egg produces a worm (*larva*) which changes into a chrysalis, when it assumes the appearance of death, and from this issues the perfect insect. All insects, except the *thysanouræ*, and those which are parasitical, (*Anoplura* of Leach), undergo these changes, to which the general name of *metamorphosis* is given.

b. The *arachnides*, or eight-footed (*Octopoda*) animals, have a head without *antennae* confounded with the chest, eight legs or paws, and no wings; most of them have feelers; they are subject to *moulting*, or change of skin, but not to metamorphosis. They present two degrees of organization. In the first and most simple, consisting of tracheal arteries, express organs of circulation are not more apparent than in the insects; and those of respiration are branching windpipes distinct from each other. The second, or more complex, consists of pulmonary or branchial arteries, and occurs in spiders, tarantulae, and scorpions. These animals have a simple muscular heart, dorsal, elongated, cylindrical, branchial or pulmonary, from which vessels proceed to the respiratory organs which are pulmonary sacs, and thence over the whole body. The sexual organs are double in each sex. Some pair several times and live several seasons. The scorpions are ovoviviparous.

c. The millepedes (*Myriapoda*) form a small group of animals intermediate between the insects, which they resemble in structure, and the crustaceous animals, which they resemble in figure. Their body is oblong, consisting of a considerable series of rings, each of which supports one or two pairs of feet. The young ones, on quitting the egg, have six feet and seven or eight rings; the other feet and rings are developed as the animal grows.

d. The organization of the crustaceous animals is more complicated. Respiration is performed by pyramidal *branchiae*, which are lamellar, filamentous, or plumiform, connected generally to the base of part of the feet, sometimes even answering the purpose of these members. Their circulation is double. The blood which has undergone respiration is conveyed to a large ventral aortic vessel, which distributes it through the body; it then is returned to another large vessel which sends it to the *branchiæ* or gills. They inhabit the water.

C. The Molluscous form a division of invertebrated animals, in which the form is generally symmetrical but without articulations. They consist of the *Mollusca acephala*, the *M. cirrhopoda*, the *M. gasteropoda*, and the *M. cephalopoda*.

a. The *Mollusca acephala* are of two sorts, either destitute of shell, or provided with a shelly covering.

The *M. Acephala* without shell bear some resemblance to the Radiated animals. Some are united into a common body like the polypi; some are of an asteroid shape, with the mouth at the circumference and the outlet in the centre; some form a cylinder in which the outlets terminate, while the mouths are open entirely; in a fourth sort the intestinal organs are prolonged in a common mass, while the radiated mouth and the outlet mutually approach near the free extremity of the body; a fifth, which continue long united after birth, have the shape of a contractile tube, open at both ends, in the substance of which the internal organs are placed; lastly, a sixth sort, which are fixed to rocks, have the shape of two tubes, one inserted into the other, in the interval of which the water is made to pass. All of these have an alimentary canal with two apertures, *branchiae* or gills, a liver, a heart and ovaries, or internal germs, which produce, without pairing, living individuals; all of them, also, have nervous ganglions and filaments.

b. The *M. cirrhopoda* form a small group of animals intermediate between the molluscous and the jointed animals. The stomach is provided with many minute cells, which seem to do the office of liver; the intestine is single; a dorsal heart, lateral *branchiæ*, a double ovary, or a mass of internal germs, a double serpentine canal for the exit of the young, and a series of nervous ganglions beneath the belly, are their peculiarities of interior structure. These animals are sessille or pediculated, but in all cases fixed.

c. The *M. acephala, conchifera*, with a shell, have the body without head, containing all the visceral organs, entirely invested by the hood folded in two, and provided with a calcareous shell, generally bivalve, sometimes multivalve. The mouth provided with lenticular leaflets covered by the hood; the outlet covered in like manner at the other end; four very large bronchial leaflets; the liver voluminous, containing the stomach and part of the intestine; the foot, when it exists, attached between the four *branchiæ;* the heart single, aortic, placed along the back; a principal ganglion situate above the mouth, united by two nervous chords to another opposite; procreation of live young going on without pairing; and, finally, one or two muscles to shut the shell, and an elastic ligament to open it,—are the chief characteristics of this order of molluscous animals.

The *M. brachiopoda*, which have two fleshy arms in place of feet, and appear to have two aortic hearts, are referred to this head. They are not numerous.

d. The *M. gasteropoda* creep on a fleshy disc placed under the belly. The hood which covers its back varies in extent and figure, and produces generally one univalve or multivalve shell. The head placed before, and more or less detached from the hood, has generally two, four, or six *tentacula* above the mouth, which serve for touch, sight, and perhaps smell. Small point-like eyes are ordinarily found either on the head, or on the tentacula. The heart is aortic and single. The other organs vary much.

The *M. pteropoda* form a small group between the *acephala* and the *cephala.*

e. The *M. cephalopoda* consist of a small class of the inarticulated or unjointed animals, the organization of which is most complicated. Like the crustaceous animals, they approach in structure the lowest of the vertebrated animals. Their organic characters are as follow. A soft pulpy body enveloped in a sac formed by the hood, which spreads laterally like fins, and through the aperture of which the head appears, with the feet or fleshy arms, with suckers for walking, grasping, and swimming; the mouth between the base of the feet, armed with two strong horny jaws; a gullet like a bird's crop; a second stomach muscular as a gizzard, and a third membranous; a single short intestine, terminating in the aperture of the sac before the neck; a double system of arteries and veins; two branchial ventricles and an aortic ventricle; two branchial organs situate in the sac where the water enters and issues for respiration; a large liver which pours bile, by two canals, into the third stomach, and separate sexual organs. The eye is formed of many membranes, and is covered by transparent skin. The ear is single,—a small cavity on each side near the brain, without outlet, and in which is suspended a membranous sac which contains a small stone. The brain is enclosed in a cartilaginous cavity, which is the rudiment of a scull.

Such is the immense series of invertebrated animals. They form three divisions or different types, in each of which there is a general resemblance, and different degrees of complication and perfection of organization. The Radiated are the most simple. Some of them approach to the infusory animals; and the most complicated have no central organ of circulation, and no predominant nervous organ. Destitute of these, they are destitute of organic or vital unity. Next to the Radiated come the Molluscous and the Articulated Animals. Their comparative organic superiority it is difficult to determine; for if on the one side the articulated animals are below the molluscous in respect of the vegetative organs and functions, since many have no real circulation which is found in all the molluscous animals, yet on the other side, the latter are below the articulated animals in development and approximation of nervous masses; and, above all, in instinct, which is so perfect in some articulated animals, that it approaches that of the vertebrated classes.

II. The vertebrated animals (ANIMALIA VERTEBRATA) constitute a type or mode of organization to which man and the animals most similar belong. By the organs of the vegetative functions, they approach to the invertebrated animals, but differ widely by those of the animal functions. Their body, symmetrical in shape, always consists of a trunk, and, with few exceptions, members. The trunk is sustained through its whole length by a column consisting of movable pieces termed *vertebrae;* which, though partly solid, form a canal for the central mass of the nervous system. The head, at one end of this column, consists of the skull, in which the brain is placed, and the face, which consists of the jaws, and certain cavities for the organs of proper sensation. The rest of the trunk, with or without certain portions of bony matter, forms one or two great cavities for containing the organs enclosed in the vegetative or nutritious functions.

In all the vertebrated animals, the intestinal canal, extending from the mouth to the opposite outlet, and presenting sundry enlargements, is provided with secreting glands; viz. the salivary, the pancreas, and the liver. In all are arteries, veins, a heart variously shaped, red blood, and chyliferous and lymphatic vessels. One class only, that of fishes, have gills, (*branchiae*); in the others the breathing organ is a lung. The organ for secreting bile receives the blood returned from the intestines and spleen by the portal vein. All have proper glands for secreting urine, and most have a bladder or receptacle for this excreted fluid. In all, the sexes are distinct; the female has one or two ovaries, from which the *ova* or eggs are detached after fecundation or impregnation by the spermatic fluid of the male. The mode and other phenomena of this process vary in the several classes.

The muscles are numerous, and, except those which form the heart, move the skin, mucous membrane, and organs of sensation, are inserted into bones which they cover and move on each other. Vertebrated animals which have a lung, have also a larynx, though all have not voice. The organs of sensation are two eyes, two ears, two nostrils, the tongue and the skin. But it is chiefly and essentially by the extent and disposition of the nervous system that the vertebrated animals are to be distinguished. We have already alluded to the central mass which consists of a thick cord, or peculiar organic matter, to which the name of *cerebral* or *nervous* has been given, enclosed in the canal formed by the vertebral pieces. To this nervous centre the presence, size, and other properties of which are constant, and distinguish these classes, other portions of definite figure, but various in size and number, are attached. These have been termed, whatever be their variations, brain, and small brain (*cerebellum.*) They are contained in that cavity of the head termed the skull, which they fill accurately in some classes, but imperfectly in others.

The author then proceeds to enumerate certain organs or organic matters, and animal fluids, which belong to particular classes, and under this head adverts briefly to the varying properties of the blood or nutritive fluid, the chyliferous and lymphatic vessels, the bones, and the serous and synovial membranes; remarks the peculiarities of the nervous system, the circulating system, the organs of respiration, motion and generation, in the different classes; and after explaining briefly the modifications of the latter function, under the heads of oviparous, viviparous or mammiferous, and ovoviviparous propagation and parturition, makes the usual divisions of vertebrated animals, and enumerates the peculiarities of each class.

A. Oviparous vertebrated animals are divided according to their respiration, temperature, atmosphere which they inhabit, kind of movements, cutaneous appendages, &c. into three classes, Fishes, Reptiles, and Birds.

a. Of Fishes, the organization is evidently intended for swimming. They are suspended in a fluid as heavy as themselves; and in many a bladder full of air beneath the vertebral column causes the specific weight of the animal to vary by its compression and dilatation. The shape of the head varies; its structure is complex either in the skull, the lower jaws, or the distribution of the teeth. The number of members varies from total absence to four. The circulation is two-fold; the mass of blood passes through a respiratory organ, but the atmosphere is aërated water. For this purpose, an organ termed *gills* (branchiæ), consisting of leaflets attached to lateral rings of the hyoid bone, and composed of many membranous plates, covered with innumerable blood-vessels, is placed at the side of the neck; its orifice is covered with a membrane and an osseous opercle. The water which the fish forces into the mouth, escapes between the divisions of the organ, and acts on the blood. The heart has only one auricle which receives the veins of the system, and one branchial ventricle. The blood having traversed the gills, is conveyed to a large vessel under the vertebral column, from which it is distributed to all parts of the body. The genital organs are peculiar. In the male, an organ termed *roe*, of great size, corresponds to the testicles of other animals, and in the female the milt is a proper ovary. In most the eggs are deposited, to be impregnated by the spermatic fluid of the male; in others, which are generally ovoviviparous, copulation and introduction of seminal fluid take place. The senses are not very perfect; the end of the muzzle contains nostrils; the eye consists of a horny, spherical plate, little aqueous fluid, and a spherical lens; the ear is composed of a vestibular sac, in which stony bones are suspended, and three membranous semicircular canals, generally situate in the cranial cavity; in some there is an oval orifice *(fenestra ovalis.)*

It is only in one genus of this class of vertebrated animals that the deviation from the symmetrical arrangement of organs is found;—in the flat fishes, both eyes are on the same side of the head.

b. The Reptiles present in configuration, structure and functions, greater varieties than any of the three other classes. The circulation is simple, and the respiration partial. In other words, the heart sends the blood into an artery, one branch of which only goes to the lung; the result of which is, that in each circuit of the blood part only undergoes respiration. The lungs have the form of sacs or large cells, in which respiration may be suspended without stopping circulation. The blood is cold, or not warmer than the atmosphere. They are divided into families or tribes, according to varieties in organization.

1. The Tortoise-family (*Chelonii*). A heart with two auricles, and receiving different blood,—and one ventricle with two communicating cavities, in which both kinds of blood are mixed. They live for months or years without eating, and survive decapitation several weeks.

2. The Lizard-family (*Sauri*) and Crocodiles. Heart as in the tortoise; ribs movable for respiration; the lung extensive.

3. The Serpent-family (*Ophides*). Heart with two auricles; no feet.

4. The Frog-family (*Batrachi*), Toads and Salamanders. Heart with one auricle and one ventricle; lungs. In early life they have gills as fishes, and a similar circulation; the artery is distributed among the gills; the vessels are again united into an aortic trunk for the system at large, and even for the lungs. This is the Tadpole state. When the gills disappear, their arteries are obliterated, except two branches, which unite and form the aorta, and each of which gives a branch to the lung. They are then frogs, toads or salamanders. Some retain the gills, and continue tadpoles during life.

c. Of Birds, the organization is obviously intended for flight; their configuration, proportion of parts, extent of respiratory organs augmenting their specific lightness, and communicating, as is believed, extraordinary muscular vigour, the arrangement of the individual parts of the skeleton, the disposition and proportion of the thoracic and ventral members, its tegumental appendages; and, lastly, the arrangement of the organs of sensation are well adapted to this form of motion through an elastic fluid. The peculiarities of interior structure are too minute for enumeration in this place.

B. The Viviparous, or Mammiferous vertebrated animals, not only differ from the oviparous in mode of generation and degree of respiration, but are distinguished by more perfect animal functions, and intelligence less guided by instinct and more capable of perfection.

The peculiarities of structure recounted by M. Beclard are greatly too numerous to allow us to repeat them, and perhaps the objects themselves are at least generally known to our readers, and therefore scarcely require our minute attention. They consist chiefly, 1st, In the form and number of certain parts of the skeleton, which are connected with the nervous system or its parts, and with the organs of proper sensation; 2d, In the arrangement and disposition of the organs of circulation and respiration; and, 3d, In the mode of generation, which is essentially viviparous. We are satisfied, that, when we mention these general characters, our readers will find little difficulty in filling up the details, which are readily derived from those objects with which anatomical readers are most familiar. They will find the sketch of our author a useful and convenient guide. The description of the peculiarities of the human subject, which is derived from Blumenbach and Lawrence, is short, but sufficiently distinctive.—

"Intelligence," says our author, "which constitutes man, is characterized by consciousness, reason, free-will, by the sentiment of moral distinctions, and by that of a divine cause. Man is, of all the mammiferous creatures, that in whom the cerebral and cerebellic hemispheres are most developed, and most marked with convolutions. The volume of these hemispheres is very considerable, compared with the vertebral portion, the nerves, the organs of sensation and the muscles. His cerebral functions are very much developed, and widely differ from instinct. He is endowed with speech, and lives in society. He is the only creature that is truly two-handed and two-footed, and his whole person is constructed for the vertical position. The heart is directed obliquely on the diaphragm, and the disposition of the aorta is different from that of quadrupeds. The organs of digestion are suited to food of various kind, and principally vegetable. The penis is free without interior bone; the womb is a simple oval cavity; the teats, two only, are placed before the breast."

III. In the second section which now follows, M. Beclard enumerates more minutely the peculiarities of the structure of the human subject; and, after various remarks on the median line, symmetrical configuration, and the division of the human body into separate regions, recognises and adopts the ancient method of dividing its component parts into solids and fluids. This and the chemistry on which it was founded, he thinks, has been too much neglected since the time of Haller and his school; and to do the subject justice, he allots it a more lengthened consideration than under the most favourable circumstances it can require. It is difficult, we think, to say in what light this scholastic, and at the best, useless division of animal substances ought to be viewed. It is certainly one of those happy examples of general proposition, which can never be charged with communicating a great degree of knowledge, or any very novel views. That the animal solids, as they have been named, ought to be distinguished from the fluids, we readily admit; that the animal fluids are not unimportant, either in a state of health or of sickness, and ought not to be overlooked, we also admit; and that the refined and delicate resources of analytical chemistry have furnished some information on their composition, and may in proper hands furnish a great deal more, we will not deny. But we do not know that a distinction, which is evidently founded on the physico-mathematical hypothesis, which formed at once the boast and the disgrace of the Boerhaavian school, ought to be retained in an age which professes to be guided by observation and experiment. It may indeed be said, that such a distinction cannot in the present day lead to any improper consequences, or bias the sound sense of modern physicians. But if it be even useless or harmless, what reason is there for retaining it, if others equally good can be obtained?

We could have wished that M. Beclard had rejected another of the ancient distinctions; which, if not closely connected with that to which we have now alluded, is at least more gratuitous, and equally useless. In his observations on the organs or animal solids, M. Beclard has laid hold of the notion of a common or elementary fibre; and has indeed admitted that it is extremely vague, and not yet intelligible. This was proper, and pleased us extremely. But he incontinently makes, by a short train of authorities, the inference that such elementary fibre really exists. First, says he, Haller admitted, in the composition of the organs, besides cellular tissue formed by union of fibres and plates, muscular fibre, and medullary substance. Then, says he, this division has been since sufficiently generally admitted, with slight modifications more or less happy. Thus Walther admits a *membranous* or *cellular tissue*, a *fibrous*, or *vascular*, and a *nervous*; Pfaff, a *vascular* or *fascicular*, and a *cellular*; others again, a *cellular*, a *vascular*, and a *massive* or *solid*—without cells and without vessels. M. Chaussier, he reminds us, added to the three component parts of Haller, a fourth, under the name of *albugineous*,—the basis of ligament; and it could not pass through the hands of M. Richerand without being augmented by the *epidermal* or horny, which thus forms a fifth. Of the twenty-one tissues admitted by Bichat, continues M. Beclard, he regarded the cellular, vascular and nervous, as generators of the others; and to complete this chapter of hypothesis, M. Meyer of Bonn, in his *Histologie*, admits three elementary organs also;—1*st*, the cell, the vessel, or the gland; 2*d*, the irritable, cellular, or muscular fibre; 3*d*, the sensible fibre, or nerve.

But even with these modifications and changes, the division of Haller does not satisfy M. Beclard. We do not thus reach the last term of analysis, which it is possible to obtain in anatomy. By the aid of the microscope, these simple organs, and all their modifications, and all their compounds, may be reduced to two anatomical elements;—an animal substance areolar and permeable, and microscopic globules similar to those of the fluids. The first substance alone forms plates, and often fibres, which differ from each other in being longitudinal and filiform in the first case, and spread out or widened in the second. When united, they form cells (*areolae*). This first element, which by itself, under different modifications, constitutes the great part of the organs, when combined with the other, the particles or globules of which it collects and unites, forms the muscular and nervous fibre.

Now, on the whole of this we have merely to remark, that it is of no consequence to the question at issue, what one great speculatist thinks, or what another great speculatist thinks, on a subject on which our knowledge is yet in its infancy. It is not by stating what division Haller attempted of the ultimate elementary fibres, or in what manner his successors modified this division, that we can hope to acquire or communicate just and useful ideas of the organic elements of the human body;—but by examining and treating the animal tissues in various modes, by recording the results with fidelity, and showing what these results indicate. We have ever regarded this division of Haller as the most unfortunate thing for general anatomy that could happen; for, whatever might be the research, judgment, and originality of this eminent physiologist on other subjects, in this he certainly suffered himself to be misled by the hypothetical opinions of the ancient physicians; and the great authority of his name has served as a pretext to perpetuate with subsequent writers, doctrines, the vagueness and fallacy of which they ought to have readily discovered. It was a gratuitous assumption, in the true spirit of mystical science; it was a scholastic proposition formed without sufficient examination of the objects predicated; it was an affirmative conclusion derived from imperfect or inaccurate observation, and without any regard to the rules of analytic induction; yet with all these objectionable qualities, it was announced and repeated by teacher and pupil, with dogmatic infallibility and presumptuous confidence. We can conceive nothing more truly ridiculous than the gravity with which the divinities of medicine delivered their dogmatic aphorisms on the nature and kinds of the *animal solid*, the *simple solid*, and the *living solid*, as if such distinctions were perceived as readily as the colour of the skin, or the shape of the limbs. We trust the anatomists of the 19th century will not testify their admiration of these worthies by beginning at the wrong end of the science, and committing the preposterous error of raising an edifice, ere they have collected materials for its foundation.

It is in truth obvious, that in matters of this description, no positive division ought to be attempted, while our knowledge of the nature of the individual organic substances is so uncertain and so scanty; and every distinction formed in such circumstances is liable to be erroneous, and therefore useless. With regard to the improvement which M. Beclard has himself attempted, after the general opinion above stated, it is surely unnecessary to make any further remark. Though not avowed, we believe it is nearly the same with that announced by D. H. Milne Edwards in his Thesis, on the authority of accurate microscopic observation. These observations are curious, and if confirmed by repetition, certainly merit the belief of anatomists and physiologists. In their present state, it is hardly requisite to say, that they are to be viewed as curiosities to be ascertained, and ought not yet to be admitted into an elementary work, as positive or established matters of fact. We are almost ashamed of saying so much against a scientific matter so trifling and unimportant to many; but it is with some feeling of pain, that we find, after most of our ancient mystical notions of anatomy had been exploded, one of the most chimerical and useless revived by an anatomical writer of some eminence. The present state of anatomical research and of chemical analysis, evidently requires a simple enumeration of all those substances found in the animal body, which can be distinguished from each other by external qualities, physical characters, chemical properties, and more obvious structure—to say nothing of physiological properties, if these are found. An enumeration or arrangement of this kind is much more instructive than any attempt at generalization, however ingenious or elegant. The profound and intimate study of the individual kinds of organic matter, ought to precede every attempt at referring these substances to a few general heads, to which they are unnaturally, if not violently compelled to assimilate themselves.

We proceed to notice very shortly, that M. Beclard, after assigning their respective merits to Pinel in France, Carmichael Smith in England, and Bichat as the imitator of both; and, adverting to the recent improvements of Chaussier, submits an arrangement of the *proximate tissues*, to use the language of chemistry, which he believes equally proper, whether the anatomical, chemical, physiological or pathological characters of the objects be regarded. 1*st*, The cellular tissue is the principal and general element of organization; it exists in every organic substance, in every organ, and is the basis of their organization. Modified in consistence, form, or in the proportion of earthy matter contained in it, it forms other kinds of organs. Arranged in the form of shut membranous sacs, it constitutes the serous and synovial system. It forms the tegumental tissue comprehending skin, mucous membrane, the follicles of both, and the generating membranes of the hair, teeth, &c. It is the same, says our author, with the elastic tissue, that recently termed *tawny or yellow*, we presume, which is the basis of the vascular system, comprehending artery, vein, lymphatic, approaching however to the muscular tissue. The glandular system, resulting from the union of the tegumental and vascular, pertains to the same order. Another modification, comprehending parts of great tenacity and resistance, is found in ligament or the desmoid tissue. Lastly, M. Beclard refers to the cellular tissue, cartilage and bone. *2d*, A second order of organs is formed essentially of muscular fibre; viz. muscles, either connected with bones, or with outer or inner teguments, or with the sensitive organs, or with the heart. *3d*, The nerves, and the central masses of the nervous system, constitute a third and last order of organic substances.

This arrangement appears to us decidedly objectionable, and founded on most erroneous principles. We cannot, in this place, enter into very minute criticism of its merits; but, with the utmost deference to the author, we submit the following remarks. If we agree with the majority of physiological writers in refering serous and synovial membrane to the cellular or filamentous tissue, with what justice can skin be referred to the same head? On what principle of structure or composition can this be done? Leaving the question of its minute structure, which we regard as still undetermined, it consists essentially of gelatine, a peculiar proximate principle, which is not found, at least to the same extent, either in serous membrane or in cellular substance. Skin is also a substance of much greater tenacity, and infinitely more resisting, than either of the other two substances; and its organization, so far as it has been yet examined, is greatly more perfect. Again, the individual parts of the vascular system, so far as they have been made the subject of attentive research, are by no means like each other, and certainly not like cellular substance. The proper arterial tunic, or the middle arterial coat, as it is often named, is a peculiar organic substance, extremely unlike any other tissue. It is said, on the best authority, that of Berzelius, to contain no fibrine, and to consist chiefly of albuminous matter in a particular condition; but we regard its chemical analysis as still imperfect. Thirdly, The structure and composition of ligament is certainly in every respect different from that of cellular substance. If it contain this last tissue, it is in minute quantity only, and in no proportion compared with the proper ligamentous structure by which it is distinguished. Its chief chemical constituent is a peculiar form of albuminous matter;—gelatine exists in it in very small quantity only. Lastly,—To refer cartilage and bone to cellular substance, is the most useless and uninstructive species of generalization we have met with, since we studied the general structure and chemical analysis of bone in the osteology of the first Monro. That learned anatomist, nearly a century ago, described this substance as composed of many *plates*, which were again made up of fibres or strings, the whole firmly joined by *claviculi* or nails; and stated, that, when exposed to strong fire in chemical vessels, they were resolved into *phlegm*, *spirit*, *volatile salts*, *fetid oil*, and a black *caput mortuum.** Such fanciful notions were more the effect of the imperfect state of knowledge, and especially of chemical knowledge, than of the judgment of the worthy osteographer; and we therefore treat them with that respectful delicacy, to which even the errors of a great mind are entitled. But to hear of an anatomist of the present day referring bone and cartilage to the head of cellular substance, is certainly a novelty for which we were not prepared. We have never regarded it established that bone and cartilage even are allied, or are modifications of the same organic substance; and some few observations and experiments which we have attempted are in complete variance with such conclusion. But it is certainly the most violent piece of generalization, with which the credulity of the student could be taxed, to consider both as mere modifications of cellular substance.

A chapter, consisting of eight subdivisions, on *Organism*, gives a view of that term similar to those of the German physiologists, from whom it is borrowed. The term may be regarded as an abstract or general denomination to express the idea communicated by the *assemblage and series of actions and processes which take place in living beings, especially animals.* In speaking of the properties of organic substances, he adds to the three varieties of irritable motion, or tonicity, vascular contractility and myotility (muscular motion), that of *vital turgescence*, to which sundry phenomena have been referred by Hebenstreit; and among the varieties of sensation, admits the *coenaesthesis* or general sensation of Reil and Hubner. The former is evidently the same with what has been long remarked under the name of erection, in parts, the vascular structure of which is known to be so constructed as to admit of a temporary extraordinary distension. For the phenomena of the second, which have a real existence, it is too obvious that we have long felt the want of a general designation; and it is well that the term of Reil, or any other equally expressive and convenient, be adopted. One thing, however, we must notice particularly. The phenomena of common sensation do not depend on the presence of nervous filaments, as is erroneously imagined by many physiological speculatists; for it can be recognised in many tissues in which no nerves can be shown to exist, and its intensity does not bear any relation or proportion to the abundance or scarcity of the nervous branches. A nerve itself, indeed, does not manifest a greater degree of this property than a ligament or a tendon which has no nerve; and it is as distinct in bone in which nervous branches are scanty, as in muscle in which they are abundant. *Common sensation, in short,* is a property inherent in every animal tissue, and pertaining essentially to its structure.

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* The Anatomy of the Human Bones and Nerves, &c. By Alexander Monro, M. D. pp. 5 & 11.

On the subjects treated in the subsequent divisions of this section,—the development and differences of organization, in which the several divisions of the human race are enumerated and distinguished by their specific characters, from Blumenbach and Lawrence, the alterations of organization, which may be either original imperfections, or the result of disease, and on death and the lifeless body,—we must be content with a general reference to the work itself.

On the details of the subsequent parts of this book it is unnecessary to enter; for it is quite out of our province to attempt, in these pages, an analytical account of an elementary work which the student must study for himself. For the accuracy and correctness of these details we can vouch with great confidence, and have almost nowhere seen any thing like want either of information, of research, or of useful and instructive description. The notices of morbid changes incident to the several tissues, are derived from the most authentic sources, both in the medical history of this country, and in that of the Continent, and are narrated in plain and intelligible language. On the whole, it may with truth be said, that the present volume contains a collection of information on the structure and diseases of the organic tissues, extremely useful to the student, and very convenient to the practitioner.

IV.

Tabulæ Nervorum Uteri. Auctore FREDERICO TIEDEMANN, Anatomes et Physiologiæ in Academia Heidelbergensi Professore, Magno Duci Badarum a Consiliis Aulæ Intimis, Societatis Physico-Medicæ Heidelberg H. T. Directore, Academiæ Scientiarum Parisiensis, Berolinensis, Monacensis, Societat. Goettingensis, Medico-Physic. Erlangens, Nat. Curios. Mosquens. Turicens. Rhenan. Marburg. Franco-Furt. Hanov., aliarumque Societ. Liter. Sodali. Heidelbergæ, MDCCCXXII. Sumptibus Augusti Oswald. Londini, apud R. Ackermann. Parisiis, apud Treuttel et Würz.

It is ever with feelings of undisguised satisfaction that we find our attention called to record the progress made by diligent and judicious cultivators of anatomy, in adding to our knowledge of the human frame. Whether it be to correct the errors of our predecessors, or to place their just views in more conspicuous lights, or to communicate information hitherto inaccessible or unattainable, or even to make discoveries, we are generally certain that our time is well employed. The work before us is a short specimen of the descriptive anatomy of an organ, the structure of which cannot be said to be well understood; and the name of its author is a sufficient reason for giving a little attention to a subject, on which it is evident he has bestowed a great deal. The work itself consists of a historical introduction, in which Professor Tiedemann gives an account of the researches of previous anatomists, from Galen down to Osiander, and of a short description of the ordinary appearance and distribution of the nerves of the womb during the pregnant state. Two engraved delineations, with outline sketches and printed explanatory references, complete the work. Our duty is therefore comprised in a short abstract of the two former, with a few incidental observations, as they are suggested to us, on the latter part.

Claudius Galen, the pupil of the Alexandrian school, and the great authority on all anatomical points among ancient writers,—was aware of the existence of nerves in the womb, and states, that, in common with those of the bladder, they were derived from the ischiadic. Whether Galen ever dissected the human body or not, as Vesalius contended, is of very little moment. It is certain that he never dissected the human womb; and as this appears clearly from the writings of Galen, it is doing him no injustice to regard his assertions on the subject as either derived from the anatomy of brute animals, or the result of mere fancy.

The founders of modern human anatomy, Mondini, Berengario, Alexander Benedict, Nicolaus Massa, and some others, have left anatomical writings of various merit. They generally borrowed either directly from Galen, or, through the medium of the Saracenic translations, from him or some other of the Grecian authors; and they have added nothing derived from personal observation on the nerves of the womb. We find, indeed, nothing satisfactory before the time of Andrew Vesalius, whose personal researches converted anatomy, from a work of fancy and copying, to a science of observation and correct description. This eminent anatomist states, "that nerves, which are distributed in slender twigs chiefly to the neck and lower region of the *fundus* of the womb, issue on each side from the nervous pairs of the *os sacrum*; that very minute twigs are ramified in its upper region, which are derived from the sixth cerebral pair, and follow the roots of the ribs; but as they are very small and whitish, and supported by fat membranes, they are with difficulty displayed by dissection."

Bartholomew Eustachi, the disciple and rival of Vesalius, appears to have had a more intimate knowledge of these nerves than his instructor. In the 2d figure of his eighteenth engraving, he has excellently displayed the great sympathetic, and has delineated issuing from the lumbar ganglions, branches which, uniting in various parts, descend along the bodies of the lumbar and sacral vertebræ to the lower region of the pelvis, where they form the great hypogastric plexus. From this plexus issue branches, which unite with the 3d and 4th pair of sacral nerves to form the nerves of the womb. It is unfortunate, however, that they are represented cut off; and as the description of the figures is wanting, it is not quite clear, whether Eustachi traced these to the organ, as Lancisi and Albinus, his commentators, are disposed to believe.

For more than a century subsequent to the time of Eustachi, anatomical research added nothing to this department of knowledge; and most writers satisfied themselves with following Vesalius, and even adopted the same language. This was the case with Realdus Columbus, Andrew Laurentius, Caspar Bauhin, Adrian Spigelius, John Veslingius, Nathaniel Highmore, Thomas Bartholin, Antonio Molinetti, and others, less generally known. Plazzoni and Regnier de Graaf, who investigated the female organs of generation in 1644 and 1678, did not entirely overlook the uterine nerves. The former represents the womb to have several slender nervous branches, mutually interwoven like network, which were distributed, not only on the inner part of the *fundus*, but also on the neck of the organ, and the more external parts. De Graaf, in his eleventh Table, represents minute nerves distributed through the organ, and others dispersed over the broad ligament, and proceeding to the ovaries and Fallopian tubes. These nerves, he says, arise from twigs or shoots of the internal branch of the sixth pair, and from the sacral vertebræ.

In the 17th century, neurograpy was enriched with numerous accessions from the anatomical researches of Willis and Vieussens. Thomas Willis, whose diligence in anatomical research was indefatigable, was the first who ventured to overturn the established doctrines in neurology. He remarked certain nerves, which proceeded from the lower mesenteric plexus to the ovaries; and described branches proceeding from it to the pelvis, where they formed a plexus on each side. With the plexuses thus formed, several nerves, derived from the *sacrum*, and inosculating with the former ones, unite; and branches from these are distributed to the adjacent parts, the rectum and uterus. Raymond Vieussens, in his Universal Neurography, briefly noticed those shoots of the lower mesenteric plexus which are lost in the coats of the rectum, the bladder, the ovaries, or female testicles, as he calls them, and the womb itself. It is to be deplored, however, that, with this accuracy of research, the delineations of the sympathetic trunk, and the uterine nerves annexed to their descriptions by Willis and Vieussens, are represented in the most erroneous manner, and the rudest style of art. They are, in short, utterly unlike the nerves of the womb in nature, and appear to be quite imaginary and fictitious.

John William Riva, a physician of Asti, in Piedmont, published the first figure of those nerves which issue from the great hypogastric plexus, and proceed to the uterine substance. These were at length, in 1712, considerably after his death, engraved by Peter Berretini of Cortona, and published with illustrative notes by Caietano Petrioli of Rome, who unjustly suppressed the name of Riva, as if they owed their existence to Berretini, though the entire praise was due to Riva as the anatomist and discoverer. The great hypogastric plexus, and the nerves proceeding from it to the womb, are represented in the twenty-seventh engraving.

The few accounts of the uterine nerves which are found in the writings of Winslow, Denys, Heister and others, are derived, not from personal researches and dissection, but are compiled from the writings of Vesalius, Willis and Vieussens. Dominico Santorini, an anatomist of diligence and considerable accuracy, only noticed the nerves which pass between the plates of the broad ligament to the ovaries and Fallopian tubes, and illustrated them by a delineation. For a more accurate description of these nerves we are indebted to Haller, who uses the following terms:

"The source of these nerves is manifold. The first and uppermost arise from the renal plexus. From this a very long nerve goes behind the left renal vein, and before the aorta downwards with the spermatic chord, is enlarged by the intercostal trunk near the first sacral vertebra, and descends into the ovary. Other nerves issue from the mesocolic plexus, and from the intercostal trunk, and from the spermatic nerves. A plexus is thus formed, which descends before the aorta and the left iliac artery, and forms a network on the intervertebral cartilage of the last lumbar vertebra, from which branches descend into the pelvis, and proceed in large trunks to the womb and bladder. Others proceed by the broad ligament to the Fallopian tube and ovary. But this plexus furnishes other shoots which unite with the fourth sacral pair, and proceed in great numbers to the womb and bladder;—a single twig from the fourth sacral pair pursues an anterior direction to the vagina. Lastly, a nerve, corresponding to that which the penis receives from the ischiadic in the male, making a circuit from the third and fourth sacral nerves, goes in like manner round the ischial and pubal bone to the extremity of the vagina and vulva and its muscles, and the clitoris, the dorsal surface of which it traverses. These nerves I have actually seen;—I do not deny that there are more, for this sketch is not considered complete."—*Elementa Physiologiæ, tom. vii. lib. xxviii.* § 49. *p.* 135.

From this passage Professor Tiedemann concludes, that although Haller had traced several nerves to the womb, he was, however, unacquainted with their course and distribution in the substance of the organ.

John Gottlieb Walter, who had spent years of industrious research in the elaborate investigation of the nerves of the thoracic and abdominal organs, published his delineations in 1783 at Berlin. He was the first who gave accurate representations of the natural situation and size of the nervous chords and filaments; and he represented, among others, the uterine nerves issuing from the fourth hypogastric plexus, and from the third sacral nerve. Professor Tiedemann regards these delineations as not sufficiently accurate for the anatomical inquirer,—because Walter entirely omitted the larger uterine nerves issuing from the great common hypogastric plexus; and did not trace the nerves into the substance of the organ. Of the twigs from the fourth hypogastric plexus, he states, that at the neck and mouth of the womb, they are so minute as to escape inspection; and the uterine and third sacral, he represents as disappearing in the body and *fundus* of the organ.

Our distinguished countryman, William Hunter, in his magnificent and elaborate work on the gravid womb, unfolded the course and distribution of the uterine nerves, much more fully and accurately than any one who had preceded him. He described faithfully the origin of the filaments from the abdominal part of the sympathetic, and the great common hypogastric plexus; but he did not remark the lower uterine plexuses, which communicate with the sacral ganglions and nerves. At length, very recently, the Gottingen Faculty of Physicians proposed, as a subject of inquiry, Whether the human womb was nervous in its substance or not? And its prize was conferred on John Frederic Osiander, the son of the distinguished accoucheur, for a commentary, in which he endeavoured to prove, by physiological and pathological arguments, that the organ was furnished with nerves. He did not see these nerves personally, nor did he attempt to search for them, for want of proper opportunity of tracing them with his own hand. This Essay, therefore, did not contribute any addition to the stock of knowledge already acquired; and to conclude the matter, Frederic Benjamin Osiander, in his Manual of Midwifery, * recently confessed that the course of the uterine nerves is not yet well ascertained.

From this view of the history of discovery on the uterine nerves, Professor Tiedemann concludes, that though sundry facts have been collected and recorded by anatomists, yet no very full or precise accounts of their origin, communication, progress, and distribution in the organ, have yet been given; and that this department of neurographical knowledge is in a remarkable degree of imperfection and rudeness. It was for reasons of this kind that he determined to attempt the task of a fuller and more accurate description than any yet given to the profession; and with this object in view, his first care was to inspect the uterine-nerves in females who had died shortly after delivery, and then to direct his researches to the bodies of women in the unimpregnated condition. After he had traced the nervous chords and filaments in the bodies of three women who had died in the first mentioned state, and in several unimpregnated females, he proceeded to make delineations and prepare for engravings; and he has, from careful examination, composed the following description, which has every appearance of accuracy and fidelity.

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* Handbuch der Entbindungkunst. Ersten Bandes erste Abtheilung. Tübingen, 1818. S. 172. § 291.

The nerves, which are distributed to the womb, ovaries and Fallopian or uterine tubes, are derived from the great sympathetic of each side, and constitute six plexiform nets. The *first* plexus, which Tiedemann terms the *spermatic*, as it is common to the ovaries and tubes, is situate on the anterior surface of the abdominal aorta, at that spot where the arteries of the ovaries or the internal spermatics, either in a common trunk which immediately divides into two vessels, or united, arise from the aorta. This plexus consists of several confluent or combined branches of the renal nerves of each side, and surrounds the small trunk of the spermatic arteries, or the individual vessels issuing separately from the aorta. The twigs interlacing with the spermatic arteries, descend into the pelvis, and proceed between the peritoneal folds, which form their broad ligament, to the ovaries and tubes, and are there distributed. Some smaller twigs accompany a branch of the ovarian artery to the substance of the womb, and unite by anastomosis with the proper uterine nerves.

The *second* and greatest plexus, which Tiedemann terms the *great upper hypogastric*, or the *common uterine*, is situate before the fifth lumbar vertebra, in the space between the primary or common iliac arteries. It consists of several branches, which arise from the renal ganglions, and from the upper lumbar ganglions of the great sympathetic. The branches to which the renal ganglions contribute, descend to the sides of the aorta, give twigs to the ureters and lower mesenteric plexus, and then, uniting with the branches of the upper lumbar ganglions, cross the iliac arteries, and are lost in the plexus. Besides these branches, several twigs derived from the third and fourth lumbar ganglion, and following a course behind the iliac arteries, go to this common uterine plexus. The nervous threads of each interlacing variously before the fifth lumbar vertebra, form a plexus of great circumference and extent. Its circuit extends from the bifurcation of the aorta between the common iliacs to the promontory of the pelvis.

This plexus divides the promontory or limit of the pelvis into two portions, which, embracing the beginning of the rectum, separate into the pelvis at the inner surface of the hypogastric artery. The nervous bundles having scarcely entered the pelvis, form plexuses on each side of the entrance, to which Tiedemann applies the name of *hypogastric*, or *upper lateral uterine*. Several nervous filaments from the first and second sacral ganglion are mingled with them. From the plexus of each side, many branches conspicuous for number and size proceed to the womb. As soon as these nervous branches reach the uterine artery, they are immediately divided into smaller twigs, and begin to accompany the serpentine course of the minute arterial divisions, and to twine round them. They are quickly separated into filaments still more delicate, and, with the minute arteries, plunge into the cellular and fleshy substance of the womb, in which they are finally lost. Of all and each of these nervous filaments, wherever they bend on the posterior and lateral surface of the womb, some sink into the neck of the organ, others ascend along its middle or body, and not a few are finally lost in its fundus.

From the lower part of the lateral hypogastric plexus, several branches descend to the neck of the womb and the vagina, where, uniting with anterior branches of the third and fourth sacral nerve, they form a great plexus, which Professor Tiedemann terms the *lower lateral hypogastric.* The filaments which enter the plexus swell into genuine ganglions, and thus form a gangliform plexus, from which filaments again issue, some to the womb, others to be distributed in the vagina, bladder, and urethra, and others finally, which are the lowest, to the rectum. The branches leading to the womb proceed with the uterine artery to the side and anterior surface of this organ, divide into numerous filaments, which interlace with the arterial branches, and communicate with the uterine nerves derived from the upper hypogastric plexus.

Such is the account given by our author of the various sources of the uterine nerves, of their course, and of their ultimate distribution in the substance of the organ. He takes occasion to draw the inference, that the nerves of the womb, derived from the sympathetic, are numerous and abundant; and that those anatomists are grossly mistaken, who assert that the organ is either destitute of nerves, or is sparingly supplied with them.

On the general appearance of these minute filaments, he informs us, that, like those of the heart, they are small, soft and reddish. But although, like them, they form frequent conjunctions with the minute uterine arteries, they do not so closely encircle the arterial branches of the surface of the womb, as the cardiac nerves surround the coronary arteries. The nervous twigs having entered the substance of the organ, suddenly disappear, and are invisible to the eyes, even when aided by good glasses. They appear to terminate, or rather to be lost and dissolved, in the cellular or mucous web which lies between the blood-vessels, the lymphatics, and the fleshy fibres. This circumstance leads Tiedemann to differ from those anatomists, who think that the twigs of the sympathetic nerve are allotted to the arteries only.

Professor Tiedemann has made an interesting observation on the varieties of the uterine nerves in young, adult, and aged subjects, and in the pregnant and unimpregnated state. The size and abundance of nervous filaments distributed to the womb, varies at different periods of life. In girls, previous to puberty, they are very small; in virgins and women, they are observed to be larger and thicker; and in old women, they are very small and delicate. William Hunter thought that the uterine nerves became larger during pregnancy. According to the observations of Tiedemann, this opinion is a matter of fact; for in the bodies of three women who had died after delivery, and which our author dissected, he found these nerves much larger than in unimpregnated women.

We have now submitted to the attention of our readers the whole of the facts relating to this department of neurography ascertained by the research of Professor Tiedemann. They are therefore to be taken on his authority and veracity as an anatomical observer, and may be, without risk, regarded as substantial accessions to the treasures of the anatomist. The subsequent remarks which he has annexed to his description are of a more questionable nature, and will be read with less confidence, and must be considered with that degree of circumspection and doubt which it is ever prudent to exercise with regard to speculative opinions. The remarks to which we allude are sundry thoughts and inferences on the use of the nerves, and on the nervous properties and sensibility of the womb. They are short, and we may therefore notice them without much injury to our more serious occupations.

We have already noticed that Professor Tiedemann regards it as established by his researches, that the womb is liberally supplied with nervous filaments, which are diffused through its whole substance. This proposition, which must be admitted, as we have above said, as a matter of fact on his authority, suggests, in the first place, the inference, which he believes to be consonant to truth, and the understood laws of the animal economy,—that the womb derives sensibility from its nerves, and performs its peculiar organic actions by virtue of nervous power.*

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* We must acknowledge here that we have been obliged to express, by circumlocution, what Professor Tiedemann has said shortly in the words "et vi nervosâ cum organismo conjunctum esse." We honestly acknowledge that this is not very intelligible, and our object in using the periphrastic mode of expression is to make it intelligible. It is evident that the obscurity of the Professor's language lies in the words "organismo conjunctum." To understand what is meant by them, it is re-

The womb, says he, is so far from being a dull and insensible organ, that it enjoys a very high degree of sensibility; and, in proof of this, he adduces several observations. When the mouth of the womb is touched in the living subject, the individual perceives it; a probe introduced also gives rise to sensation. Haller remarked, that its mouth, during the venereal act, becomes the seat of a pleasurable titillation, which he ascribed to friction by the organ of the male. A woman is deprived of the sexual appetite by excision of the womb,—according to the testimony of Wrisberg, that a woman, in whom a midwife had performed this operation, though, after healing, she had commerce with a male, was yet unconscious of her former sensations. Lastly, Tiedemann remarks, that wounds of this organ are violently painful, on the authority of Rousset, who states, that excision of a prolapsed *uterus* was attended with much pain; and of Wrisberg, who mentions, that in the instance already quoted, most acute pain was suffered, while the midwife was cutting off the depending organ from the genitals.

To demonstrate the influence of the nerves over uterine organism, Professor Tiedemann adverts to the changes which they undergo during pregnancy. Since these nerves, he remarks, become larger when the womb is gravid, no one can deny that the force of sensation is at this time increased. That the motions of the fœtus are felt, must also be ascribed to its sensibility. A pregnant woman feels the hand of the midwife when turning the infant. And, lastly, the pains attending and following delivery, appear to proceed from contraction of the

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quisite to be aware of the sense attached to the former of these words among the German physiologists; and it is to be deplored, that, on too many occasions, they evidently show, that they use the term without understanding exactly what they mean. It is one of those unfortunate abstractions about which they have reasoned and debated for many years, without ever giving it a precise definition; and whoever reads four or five German writers who often use the term, will find, that neither do all of them use it in the same sense, nor does the same writer invariably attach to it the same signification. In the present case, we conceive it is not to be doubted, from the context, that Professor Tiedemann employs it to express that general property or assemblage of properties, if you will, which belong to any organ as a vital tissue. The *organism of the womb*, according to this view, means the circulation and sensibility,—the property by which it grows, as the individual grows, discharges menstrual blood, is susceptible of impregnation and conception, enlarges with the enlargement of the fœtus, and at a definite time expels this body, contracts, and returns to its former dimensions. In short, the *organism* of the womb is employed to denote every property and action of that organ as a part of an animal body. Having premised this explanation, we conceive it is easy to see that Professor Tiedemann, though confused by the peculiar notions of his countrymen, means neither more nor less than what we expressed by circumlocution in the text, that it *was allied with organism by means of nervous power*, or, in other words, that it owed its *organism*, that is, its organic properties, to nervous power.

fleshy fibres by which the uterine nerves are stretched and compressed.

Again, he thinks it undoubted, that the nerves exercise *some* power over the involuntary or automatic motions of the organ; but the reason assigned for this is not so conclusive, viz. because probably the nerves render involuntary muscles susceptible of impressions or *stimuli*, and dispose them by contractile irritation to motion. But he hazards even a bolder inference still, which consists almost entirely of facts, which are either assumed or by no means demonstrated. When it is certain, says he, that the fleshy fibres arise during pregnancy, it is not unreasonable to think, that the motions of the womb, by which the fœtus is expelled during delivery, depend on, and are guided by, nervous power.

Though these doctrines of Professor Tiedemann are by no means unlike many which are deliberately advanced in physiological writings in general, and certainly do not offer much greater violence to the reason than many others do, yet we cannot say that we are quite prepared to admit them in their present form. We neither doubt nor deny the influence which the nerves of the womb probably exercise over its properties and actions; but we do not think that a sufficient number of well-established facts has been collected concerning the phenomena of uterine action, to justify the confident inferences which the learned Professor draws. The facts regarding the sensation of the organ, as shown by touch, injury, wounds, &c. prove nothing more than that the womb is possessed of the usual properties of organic substances,—and that the individual still retains the consciousness of these properties. But we do not conceive that a necessary inference from this is, that it owes these properties to its nervous filaments. Many facts tend to show, that the sensation of pain especially depends not on the presence of nervous filaments in the part affected, but on properties originally inherent in its tissue, as an organic substance. The sensation of pain is in no case exactly in the ratio of the nervous filaments distributed to each tissue; and, in many tissues, in which no nervous filament can be recognised, pain takes place as in those in which nerves are abundant and conspicuous.

The dependence of the sexual sensations on the uterine nerves is, in our opinion, a very untenable doctrine; and is, moreover, contradicted by numerous facts which are established on competent authority. It is certainly quite natural to think, that a female, from whom the womb has been partially or entirely removed, will not retain these sensations in the same perfection in which she originally felt them, when the organ was complete; but, this fact, if it be admitted on the small number of authenticated cases, proves, not that these sensations depend either on the uterine nerves, or on the nervous power of the organ, but that they are connected with the presence of the great organ of female conception. The inference is further contradicted by the well known fact, that extirpation of the ovary in the female, either of the human subject or of brute animals, extinguishes the sexual appetite completely. In this instance, the womb and its nerves are left entire, yet the individual exhibits no proof of sexual appetite, or susceptibility of venereal sensation.

The most curious and embarrassing part of the subject of these speculations yet remains. An organ, which has long periods of inaction, or absolute torpor, which, in all, remains a considerable time in a state of quiescence, and is fitted for its proper functions only during a certain period of life; and in many, at certain times only of that period, constitutes a curious and interesting subject of inquiry. Are we to suppose that its nerves actually become large, more numerous, and more sensible in the periods of its action, than they were in those of its quiescence? By what means can they become larger, or more sensible; for it is scarcely within the compass of belief that their number can be increased? Do these nerves undergo an actual enlargement during each pregnancy; and do they, with the same regularity, shrink to their previous minuteness during the intervals of pregnancy? On these points, notwithstanding the researches of Professor Tiedemann, we still are in want of some good and well established facts. They might be ascertained to a certain extent in the bodies of brute animals in the first instance, which might thus lead to their confirmation or refutation, by subsequent opportunities of research in the human subject. At present, it is obvious that they are so undetermined, as to be credited by none, whose credulity is not unparalleled.

We have one word only to say of the engravings. They are sketched by Professor Roux, a colleague of the author, and engraved by Duttenhofer and Karcher. Of their anatomical accuracy it does not become us to speak, further than to say, that all the parts which we have compared with our own dissections, are distinctly, and, on the whole, accurately represented. We have not had opportunity of examining the nerves of the gravid womb. The first engraving represents the nerves of the womb, Fallopian tubes and ovaries in the body of a woman of 22, who died of nervous fever six days after delivery. It gives us a front view of the spermatic plexus, the great upper hypogastric or uterine plexus, the lateral uterine plexus and their ramifications following the tortuosities of the uterine arteries. The second gives a lateral view of the pelvic nerves in general, especially the lateral uterine, and the lower lateral hypogastric filaments. As works of art, they are certainly respectable,—much superior to any thing met with in this country, but greatly inferior to the engravings of Anderloni in the works of Scarpa. The chief mistake is an occasional violation of perspective, and an unsuccessful attempt at fore-shortening. Notwithstanding these defects, however, as mere representations of the objects, they are good, and must be useful to the anatomical student.

PART III.

MEDICAL INTELLIGENCE.

DOMESTIC.

Report of Diseases of Birmingham, from April 3d to July 3d, 1824.
By John Darwall, M. D.

ACUTE DISEASES.

	No.		No.
Cephalalgia	41	Colica	4
Convulsiones	4	Gonorrhœa	4
Hydrocephalus	3	Menorrhagia	11
Iritis	1	Podagra	5
Odontalgia	1	Typhus	8
Otalgia	1	Synochus	12
Catarrhus	4	Febris Infantum	2
Cynanche Tonsillaris	5	Erysipelas	2
——— Trachealis	3	Pompholyx	1
——— Parotidæa	2	Rubeola	21
Pneumonia	6	Variola	2
Bronchitis subacuta	1	Purpura Hemorrhagica	1
Pertussis	4	Purpura Urticans	1
Pleurodyne	4	Urticaria	1
Hæmoptysis	13	Abscessus	1
Hæmatemesis	2	Phlegmatia Dolens	1
Peritonitis	3	Ptyalismus	1
Diarrhœa	16	Mastodynia	1
Cholera	3		

CHRONIC DISEASES.

	No.		No.
Cephalæa	34	Hydrothorax	2
Hemiplegia	1	Phthisis Pulmonalis	68
Paralysis Partialis	10	Palpitatio	1
Epilepsia	6	Dyspepsia	170
Mania	4	Asthenia	12
Hypochondriasis	1	Hysteria	9
Chorea	2	Gastrodynia	39
Otorrhœa	2	Pyrosis	2
Hemicrania	3	Obstipatio	79
Amygdalæ Tumidæ	2	Tenesmus	1
Tussis	16	Hemorrhoides	1
Asthma	72	Dysenteria	4
Bronchitis C.	18	Hæmatorrhœa	2

Tænia	4	Syphilis	3
Lumbrici	3	Cachexia Syphiloidea	2
Marasmus	60	Bronchocele	3
Rachitis	3	Tumor Mammæ	3
Struma	16	Acne	1
Tumor Abdominis	3	Lichen	2
Splenitis C.	6	Strophulus	6
Hepatitis C.	14	Porrigo Favosa	9
Icterus	5	Porrigo Furfurans	2
Catarrhus Vesicæ	2	Aphthæ	2
Dysuria	6	Impetigo	1
Hæmaturia	1	Sycosis	1
Calculus	3	Ecthyma Vulgare	1
Leucorrhœa	3	Pityriasis Rubra	1
Amenorrhœa	13	Nævus	1
Prolapsus Uteri	3	Psoriasis	1
Hernia Humoralis	3	Ulcus	2
Rheumatismus C.	24	Anasarca	4
Coxalgia	2		
Lumbago	10	Total,	985
Paralysis Plumbea	1		

The weather during the present quarter has been generally mild, with a fair proportion of rain. Towards the latter part of it the temperature was comparatively low, with NE. winds and much rain.

The most striking feature in the present Table is the great number of the affections of the head, forming more than a tenth of the whole, while in the former reports they have seldom exceeded one thirteenth. They varied very considerably in the severity of the symptoms. Not unfrequently there was much difficulty of motion, without pain; at other times rheumatic pains were severe. The accompaniment of general uneasiness, and often great pain, has been mentioned by many writers as characterising the early steps of hydrocephalus; but I am not aware that connection between those disorders of the encephalon and rheumatic pains has ever been noticed. Dr Cooke, who appears to have collected every thing of importance upon the subject, has not remarked it. My experience would lead me to believe that there is often a common cause for both maladies. In a gentleman whom I had the opportunity of watching very minutely for two years before his death, rheumatic pains in the shoulder and loins were very troublesome. He was at length attacked by paralysis; and a week or two before he died from an apoplectic seizure, he complained of much suffering and stiffness in the loins. A similar train of symptoms was observable in the instance of hemiplegia in the present list. Two years ago, the patient, a woman about forty years of age, had lost the use of the lower limbs for four months, from what was termed rheumatism by her medical attendants. When I saw her in the beginning of the year, she had headach, with pain in the shoulders. A more remarkable instance, however, has occurred in a woman who is still under my care. Her first complaint was excessive headach, fixed principally at a spot a little above the outer angle of the left eye; and at one time she had great dimness of sight. By the application of leeches and purging, the pain was removed, and for a week or ten days she seemed free from complaint. She then returned with most extreme pain in the right arm, which she represented as so severe as to prevent her taking any rest. The slightest attempt to move the arm was productive of great suffering, referred chiefly to the deltoid muscle, but affecting the whole arm very considerably. For this affection acupuncturation was employed, and with complete success; but in a very short time after, the headach returned. This was removed by the former remedies, but was succeeded by another attack of pain in the arm, which was likewise relieved a second time by acupuncturation. These two affections have alternated several times since in a similar manner. Many other instances might be adduced, from the present Table, of similar coincidences.

The instance of purpura hemorrhagica recovered with purging. The patient, a woman forty-nine years of age, had extensive patches of purpura upon different parts of the body, and bled very profusely from the mouth and bowels. I had seen her a week before the purpura appeared, when she complained of common dyspepsia. On the third day I ordered her a little wine; but this evidently aggravated the disease, and produced considerable headach, and was consequently not repeated. On the fifth day, the catamenia appeared, and were very copious; and from this time she gradually recovered. Her bowels were freely acted upon the whole time.

The case of purpura urticans was in a child, and recovered under a similar treatment.

The system of purging in chorea and epilepsy has become now too general to need farther recommendation. The enumeration of fresh and successful cases, however, may still serve to confirm the propriety of the plan. Both the cases in the Table recovered under active purging every other day for a fortnight.

One of the cases of obstipation was of very long standing. For three or four months the individual constantly vomited, and the whole symptoms were such as to raise a suspicion of organic disease of the stomach. Active aperients, however, completely restored her health.

Three fourths of the cases of marasmus were the sequelæ of measles; and the prevalence of the latter disease may be better inferred from this than the number in the Table. All the instances of pneumonia were derived from the same source. In the very early stages, *i. e.* a few days after the disappearance of the eruption, the inflammation of the lungs was in general readily curable; but if much longer time was suffered to elapse, the malady put on a chronic form, and almost without exception ended fatally.

There have been very few cases of fever in this quarter.

On Amputation. Letter to James Syme, Esq. from Mr Robertson, Surgeon of the Convict Hospital Ship, Sheerness.

DEAR SIR, 18*th August*, 1824.

AN opportunity having been offered in this hospital of putting into execution the mode of amputation recommended by you in the LXXVIIIth Number of the Edinburgh Medical and Surgical Journal, I determined on adopting it. My patient, a lad of 18, labouring under an enlargement of the bones of the knee-joint, which had resisted repeated local bleeding by leeches and cupping, issues, blisters, embrocations and moxa, together with several courses of alterative medicine, submitted to the operation on the 2d inst. My assistant, Mr Bayley, having undertaken to command the femoral artery by pressure with his thumb, I followed your directions in every particular, employing neither tourniquet, tenaculum, nor retractors; and, in comparison with the former mode of amputation, this was the work of a moment, with a great diminution of pain, little or no hemorrhage, and with a surface that enabled every vessel to be seen on the instant. The stump has healed by the *first intention*, with the exception of the opening mode by the ligatures, and a spot on the lower surface.

I had anticipated, from the very angular direction of the incisions, that the limb would have been too pointed, but it proves a round, full, compact stump; and, in the opinion of the naval and military surgeons, as well as that of my private friends who have attended the case, is very superior to those which result from the circular incisions. A pupil of M. Dupuytren saw it on the eighth day, and was much struck with the remarkable contrast between its uniting condition, and the charpee-covered stumps daily seen in the Parisian hospitals.

Your method has my decided and unqualified approbation, and I hope you may have the gratification of seeing it speedily adopted throughout the surgical world. The non-employment of the tourniquet is the point on which opinion will be the most difficult to eradicate, it having hitherto been considered as only to be laid aside from stern necessity. Some of my friends recommended that in this instance it should be left loose on the limb, and others that it should be kept in hand in case of being wanted; but I venture to assert, that neither those, nor any who may hereafter witness the small amount of blood lost, will hesitate one moment as to its rejection.

My colleague in hospital duty, Mr Cullen, suggested the employment of the tailed bandage (with centre piece sufficiently long to supersede the cross pieces usually put over the face of a stump) instead of the circular roller. I found it very advantageous, enabling my assistant to remove the dressings without raising the stump from the pillow, and to apply pressure in a more uniform and easy manner. I am, dear Sir, your faithful Servant, ARCHIBALD ROBERTSON.

P. S.—A convict on whom I had amputated some time ago, stole unnoticed into the Ward, and witnessed this operation. He was so struck with the rapidity of the process, and the diminution of pain to the sufferer, that he stopped me on deck to express his surprise at the *unnecessary* pain to which he had been subjected! I quieted his vexation by telling him, that this mode was not then known.

Remarks by Mr Syme.—I feel much obliged to Dr Robertson, with whom I have not the pleasure of being personally acquainted, for sending me this notice of a case so favourable to the Flap Operation; but regret that he should have said nothing of Mr Liston's merits, as his not doing so may lead those who have not read my paper on amputation to imagine that I claim more than belongs to me.

Any credit which I deserve is not for inventing the operation, since it was invented long ago by many different people, nor for practising it, since, although others have performed it occasionally, Mr Liston was the first in this country to adopt it in all cases; but for recommending it to the public by such arguments, and comparisons with the methods usually followed, as to awaken the attention of the profession. To Mr Liston, I may add, belongs, without dispute, the honour of abandoning the tourniquet in this country, his reasons for which will be found in the LXXVIIIth Number of this Journal. For though some surgeons on the Continent, as well as others in our own army, have recommended, by precept and example, the disuse of any mechanical contrivance for restraining hemorrhage during amputation, I am not aware of any surgeon in Great Britain introducing such a proceeding into private practice previous to the time of Mr Liston.

12, *Dundas Street*, 31*st August*, 1824. JAMES SYME.

Trial for Poisoning with Corrosive Sublimate.—For the particulars of the following case we are indebted partly to the daily papers, but chiefly to the kindness of a gentleman who was personally concerned in the trial; and we believe our readers may rely with confidence on the accuracy of all the facts we shall have to mention. The case is deeply interesting to every practitioner. It has excited a great deal of speculation in the neighbourhood of the place where it occurred, both on account of its complexity, and by reason of the station of the person who was charged with the crime. It turns chiefly on the medical evidence; which, therefore, occupied a great proportion of the time spent in the trial. And we have great pleasure in declaring our opinion, that almost the whole of the evidence is unexceptionably good, does great credit to the individual who was the chief crown witness, and furnishes an excellent example of the decisive information which may be supplied even in the most complicated cases by a person qualified to conduct medico-legal inquiries.

Mr Hodgson, a surgeon in Sunderland, was indicted on the 18th of August, at the last Durham Assizes, for administering poison to his wife, with the intent to murder her. The history of the woman's illness is the following: At the time the attempt was supposed to have been made, she was labouring under an attack of acute rheumatism; on account of which a physician, Dr Brown of Bishop-Wearmouth, attended her nearly from the beginning. When he first visited her, he found her mouth slightly affected with calomel. He ordered the *vinum colchici* to be taken, with occasional anodynes and laxatives; and a few days afterwards, in consequence of her being seized with considerable pain about the side and stomach, he bled her, and directed a blister to be applied over the stomach. These accessory symptoms soon subsided, the affection of the mouth also went away, and, at length, when her rheumatic complaints alone remained, he ordered the calomel to be resumed in the dose of six grains every evening, with two or three grains of opium made with it into a bolus. Laxatives were also given occasionally in the morning. She had taken three, or at the utmost four doses of the calomel and opium with some relief to her complaints, and without experiencing any unpleasant sensation in swallowing them,—when, on the 6th of June, about a fortnight after Dr Brown was first called to attend her, she was attacked immediately after taking the same medicine with violent burning in the throat, gullet and stomach. Upon this occasion she had desired that the bolus should be divided into three pills, which was accordingly done. The first of them produced so painful a sense of burning, that she expressed her belief that some mistake had been committed; but she was nevertheless persuaded to take the other two, which immediately increased the burning sensation to such a degree that she cried out. In about five or ten minutes a draught was given her, after which she felt sick; and in a few minutes more she began to vomit with violence. Dr Brown was accordingly sent for, and arrived about an hour or an hour and a half after she had taken the pills. He found the pulse small and frequent, and the skin bedewed with a cold, clammy sweat; she complained of a constant and severe sense of burning in the fauces and throat and along the course of the gullet down to the pit of the stomach; she vomited at short intervals large quantities of mucus, and rejected every thing she drank; but the bowels were not then affected. Dr Brown, suspecting that some mistake might have been committed, inquired whether it was possible that the person who compounded the pills could have used tartar emetic instead of calomel, but found that this was almost impossible. He ordered an anodyne draught to allay the irritability of the stomach; it was compounded by the prisoner and swallowed by the patient in Dr Brown's presence; but was almost immediately discharged again. He then ordered a simpler anodyne draught, consisting merely of laudanum mixed with two drachms of water. The prisoner went down stairs to prepare it, and brought it into the patient's room, where Dr Brown had remained. The latter, on taking the glass from the prisoner, remarked that the draught was "more than twice" as bulky as it should have been, and likewise more turbid; he proceeded to taste it, and was astonished to find that it had an acrid abominable taste, like that of corrosive sublimate. His suspicions naturally took a different course, on making this discovery; and on the presumption that the same substance might have been taken in the pills or draught administered before his arrival, he gave her white of eggs as an antidote. From this remedy she experienced immediate and decided relief. Owing to some unaccountable and most unjustifiable omission on the part of the King's counsel and of the judge, the patient's history was not traced any farther on the trial. But we know from the best authority, that next morning she was affected with slight diarrhœa, and on the third day with ptyalism; but she gradually recovered from her dangerous symptoms, and was able to give evidence when the prisoner was tried.

The draught which Dr Brown received from the prisoner, and which he suspected by its taste to contain corrosive sublimate, was preserved and carefully analyzed by a practised chemist, in presence of the Doctor and two surgeons, who saw the patient on the first evening of her new illness. Carbonate of potass produced in it a pale brick-red sediment, ammonia a brownish-white precipitate, lime water a yellowish-brown precipitate; and when acted on by galvanism, it amalgamated gold. These results left no doubt in the minds of the gentlemen present, that corrosive sublimate was contained in the draught; and the quantity was estimated (by what means is not stated) at 13 grains and 11-13ths. Part of the matters vomited was also subjected to analysis, but no mercurial substance was detected in them. These matters, however, were not preserved till the white of eggs had been administered, and only after the patient had been vomiting incessantly for more than an hour. Little evidence was procured of the actual composition of the pills she swallowed. A minute portion was found by Dr Brown and the two surgeons on the slab on which it was supposed they had been compounded; and these three gentlemen, together with the prisoner himself, agreed that it tasted like corrosive sublimate; but the small portion they found was consumed in tasting it, so that no chemical test could be applied to it. No information could be procured of the nature of the draught administered before Dr Brown's arrival.

Such are the medical facts of the case. Before offering any comments on their import, and the light in which they were viewed by the medical witnesses and the court, we shall give an abstract of the moral evidence also. Mr Hodgson appears, by the testimony of many of his intimate friends and of his servants, to have uniformly conducted himself towards his wife with great humanity, "and with that affection which should always be wished for between man and wife." From the first period of Dr Brown's attendance on his wife, he had been in the habit of giving Mr Hodgson his prescriptions verbally; and it appears that the latter, in compounding the medicines prescribed, had been in the custom of altering slightly the ingredients and their proportions. This was generally told to Dr Brown, who did not disapprove of the changes, as they were immaterial. On the evening of the 6th of June the apprentice who had made the former boluses with six grains of calomel and from two to four grains of opium, was told by his master to make one with eight grains of the former and half a grain of the latter. About ten, his master came to him in the shop and told him he was not to sleep there as usual that evening, because the two servants who slept with a child were incommoded by the heat of the weather, and one of them was to occupy his bed. The boy accordingly went home. The bolus was brought to Mrs Hodgson's room by the apprentice a few minutes before the preceding conversation took place between him and his master. Not long afterwards, Mr Hodgson entered the room and took away the bolus, without making any observation. No one saw what he did with it. The apprentice had left the shop by that time; and the servant who was to sleep in it, and had gone down to make the bed, did not see him do any thing; but she was not always in such a situation as enabled her to see what was done at the marble slab; and, besides, she left the shop soon after the apprentice, so that the prisoner, who had left it before her, might have returned unseen. Not long after he took the bolus from his wife's room, he returned with it, saying he had put half a grain more of opium into it. She then asked him to divide it into three pills, which he did accordingly. He gave her them himself, one after the other; and when she expressed a suspicion after taking the first, that some mistake had been committed, he assured her they were quite right, and that " she must keep it (the first) down, or she would have the same to take over again." Afterwards, when she complained of increased pain, so that she was obliged to cry out, he gave her a draught of his own compounding, not ordered by her physician. Finding herself getting still worse, she desired him to send for her mother who had left the house about half an hour before. He made some slight opposition to this, but nevertheless allowed his wife to send the servant for her. Soon afterwards he went himself for Dr Brown (at whose suggestion does not appear from the Report, although surely that important point must have been ascertained.) He told Dr Brown his wife had been seized with symptoms of inflammation in the stomach. On arriving together at the house, Dr Brown expressed his suspicion that tartar emetic might have been used by the boy instead of calomel; but the prisoner said he had perfect confidence in the boy's exactness in compounding medicines. When the Doctor discovered that the second draught contained some acrid substance, the prisoner tasted it and agreed that it tasted like corrosive sublimate. He then attempted to give some explanation as to the presence of corrosive sublimate in it, observing that he had been preparing an injection for a sailor, which contained five grains to the ounce of water. " He repeatedly requested that the draught might be destroyed;"—" and something was said about saying nothing more about it in the event of Mrs Hodgson recovering;" but the witness, who stated this, was not allowed to explain himself. When the surgeon, who was then sent for at Dr Brown's request, arrived, the prisoner observed to him, that " he considered Dr Brown had very unnecessarily caused a great deal of alarm in his family;" and repeated to him his wish that the draught should be destroyed. A second surgeon afterwards arrived. When the three medical attendants went with the prisoner to the shop and tasted the matter on the slab where the bolus had been compounded, he agreed with them that it tasted like corrosive sublimate, Finally, when the constable arrested him, he observed " it can't be helped now," and accompanied him quite freely. On his examination before the magistrate, he said " it must have occurred through mistake, as the bottle containing the powdered opium, and that containing the corrosive sublimate stood together; and he was in a state of intoxication at the time."

The following points of evidence were likewise adduced, relative to the possibility of the medicines in the shop being confounded—of the prisoner being drunk—and of his being insane. The powdered opium and corrosive sublimate stood together; the calomel (in a state of powder) in a bottle separated by a partition from the corrosive sublimate, which was in crystals; and the tartar emetic in a totally different part of the shop. One of the servants deposed that her master was drunk; but all the other witnesses, seven or eight in number, agreed that he was sober. His wife thought him insane for six months before, because "he used to go about the house with fewer clothes on him than he ought to have;" but no other evidence was adduced to this effect.

It is apparent that the medical and moral evidence are so interwoven in the case now stated, as to render their separation very difficult. In the following remarks, we shall keep them as much apart as possible.

1. Was poison given in the bolus? This question, we apprehend, will be answered in the affirmative by every person who takes an extended view of the symptoms of poisoning and of natural disease. No natural disease could produce a sense of burning from the throat to the epigastrium, *so very sudden and so very acute:* And that it arose from some acrid substance being swallowed, is rendered even more distinct by its being aggravated by each pill, and by its occurring immediately on their administration. We say *immediately*, not in the vague sense in which the word is used by witnesses even of the medical profession,—but in its most decided signification; for it is probable that but a few seconds would intervene betwixt the swallowing of each pill.—Several of the witnesses allowed that bile might have caused the sense of burning. But this was out of the question in the present case, as Dr Brown very properly mentioned. Bile would not cause a sense of burning so very acute; and besides, the sensation existed before the patient began to vomit. This appears, therefore, to be an instance in which, contrary to the opinion of Orfila and other medical jurists, a witness might swear to the fact of general poisoning from the symptoms only. [See our Number for last April. xxi. 411.]

2. Was poison given in the first draught prepared and administered by the prisoner? It caused immediate vomiting; but this is no proof of its having contained poison, and no other was adduced.

3. What was the poison in the bolus? In answering this question, it is requisite to pay some regard to the moral evidence. Suspicions being entertained, the answer must be given, if possible, with a view to these suspicions.—It appears highly probable, if not almost certain, that the poison was corrosive sublimate, and that it composed the greater part of the bolus or pills. This is shown by the violent sense of burning suddenly produced, such as scarcely any other solid poison could produce; by the speedy relief derived from the white of eggs; by the subsequent ptyalism; by the taste of the stuff on the marble slab where the bolus was compounded, or re-compounded; and by the fact that this poison was very nearly given a short while afterwards,—whether by accident or design, we do not at present inquire. Most of these grounds of opinion, especially the ptyalism, are singly equivocal, we admit; but, taken conjunctly with each other, and with the fact of general poisoning being already proved, they form a basis sufficiently substantial. When the case was first stated to us, we were disposed to think, that decisive evidence might be procured of the nature of the bolus, by learning whether the patient did or did not perceive the horrible and undisguisable taste of corrosive sublimate. But we must change that opinion. Our correspondent, to whom we applied for information on the subject, says, that she was so completely engrossed by the exquisite sufferings of the moment, that her physician could never get a clear answer from her concerning the taste of the pills. We shall assume that she did not perceive in them the powerful, acrid taste of the poison suspected. Had she been taking any article of food, or any thing but medicine, then we should most probably have said that corrosive sublimate could not have been taken. But many persons either purposely or involuntarily use means for preventing the taste of *medicines* being perceived. This may be effected either by holding the nose, or by bringing the *velum pendulum palati* over the internal opening of the nostrils. The latter mode, if practised dexterously, will enable one, as we have often personally experienced, to swallow untasted the most disgusting compost a physician could invent. Indeed, we have sufficient practical proof of the possibility of tastes being not perceived, in the frequent accidents arising from oxalic acid being mistaken for Epsom salt. No one would drain a bowl of strong oxalic acid if he perceived its taste.

4. But was there sufficient evidence that the *third* draught contained corrosive sublimate? The precipitates were indistinct in colour, as was to be anticipated from the draught containing a little laudanum; still they approached in appearance

to their characteristic colours; and the inference that they indicated corrosive sublimate, was rendered good by the effects on gold, with the aid of galvanism. But why did not the experimenters evaporate a little of the solution, and use the test of sublimation? Why did they not try the sulphureous test? Why did they not prove the existence of hydrochloric acid, and thus show that the mercurial preparation could be nothing but corrosive sublimate? As their experiments stood, it might have been any other soluble salt of mercury as well as corrosive sublimate. These, to be sure, are also poisonous; but we must reprobate every appearance of want of precision in medico-legal inquiries.

5. Could the corrosive sublimate have been administered by mistake? This is properly a question which the jury alone should answer; but in this particular case it is one which requires much medical knowledge to answer correctly; at least a great deal more than either the jury could or the judge *did* possess. We shall therefore offer a few remarks upon it. If a mistake did exist, it must have been a systematic one, since the same substance was used twice. Hence the prisoner must have first mistaken corrosive sublimate in the crystalline, or powdered state, for calomel, or powdered opium; and then he must have mistaken a strong solution of sublimate for water. Dr Brown shows, that the *first* was an unlikely error. Mistaking corrosive sublimate for powdered opium was out of the question; and mistaking it for calomel was unlikely, as the former was in crystals and the latter in powder; and, besides, they were not so placed in the shop as to render a mistake probable. The *second* mistake was not dwelt on very fully at the trial: at least no circumstances regarding it were pressed for, except such as favoured the prisoner. Thus the medical witnesses agreed, that Mr Hodgson, being a man of information, must have known, that the draught he brought would strike the physician as differing in quantity, and perhaps even in appearance, from the one ordered; and likewise, that it was such as would be at once vomited by the patient, and consequently do little harm. We need not offer any remark on these statements; they are evidently of no weight, and cannot lead to any sound conclusion as to the prisoner's motives. But it is singular, that no attempt was made to prove the possibility or impossibility of the solution of corrosive sublimate being mistaken for water. Did the prisoner keep a solution in his shop? It appears not; for when he attempted to explain the supposed accident, he said, "He had *prepared* one for an injection for a sailor." Could he have taken part of that injection? No. It was not strong enough. It contained but five grains to the ounce; and that which his wife was on the point of taking, supposing it to have been even thrice, (instead of "more than twice"), as much as what Dr Brown ordered, must have contained about 18 grains and a half per ounce. Mr Justice Bayley, following the humane practice among the English Judges of acting as counsel for the prisoner, suggested in his charge to the jury, (as we are informed by our Correspondent), that the corrosive sublimate might have got into the draught by having been accidentally dissolved in the laudanum. The absurdity of this conjecture may be forgiven in consideration of the principle which prompted him to hazard it. Of late, however, we have observed many parallel occurrences. It would be desirable, that when the Judge undertakes to make the physical sciences square with his ideas of the case, he would use more mercifully his privilege of infallibility. On the question of the prisoner's motive, we do not consider it necessary to offer any farther remarks. We have noticed it, because it hangs partly on medical evidence; and to this view of it we have confined ourselves. The prisoner was found *Not Guilty;* and therefore it would be equally indelicate and unnecessary to view the question with a reference to the other grounds of judgment.

We recommend the case to the careful consideration of every student in legal medicine. The evidence, we repeat, is, on the whole, admirably correct; and we are assured it has been faithfully reported. The report we have followed is in the London Courier for August 21st.

Medical Graduations at Edinburgh.

On the 1st August, the Senatus Academicus of the University of Edinburgh, conferred the degree of Doctor in Medicine on the following Gentlemen (110 in number), after they had gone through the appointed examinations, and publicly defended their respective Inaugural Dissertations:—

Of Scotland.

John Agnew, . . De Hepatitide.
James Anderson, . — Hydrocephalo Acuto.
William Bogie, . — Scarlatina.
Robert Brown, . — Respiratione.
George Burton, . — Inflammatione.
William Cullen, . — Remediis in Phthisi Pulmonali.
Hugh Donaldson, . — Apoplexia Sanguinea.
Gilb. Finlay Girdwood — Rheumatismo.
Geo. Gregory Grant, — Effusionibus Pleuriticis.
Ja. Craufurd Gregory, — Hydrope Acuto.
Archibald Hair, . — Typho.
Archibald Inglis, . — Variola.
Quintin Jamieson, . — Pneumonia.
Æneas M'Andrew. — Corde Ampliato.
John M'Kenzie, . — Carcinomate.
John M'Laren, . — Febre Flavo.
Alexander M'Queen, — Dysenteria.
John M'Rae, . — Hæmorrhagia Uterina.
William Malcom, . — Phthisi Pulmonali.
James Mitchell, . — Tetano.
Francis Ogston, . — Volvulo.
Samuel Alex. Pagan, — Rheumatismo.
William Reid, . — Nupera Febris Continuæ Disciplina.
James George Sim, . — Hepatitide Acuta.
Thomas Spens, . — Neuralgia.
John Taylor, . — Glandula Thyroidea.
William Thomson, . — Scarlatina.
James Thomson, . — Quibusdam Plantis Medicinalibus.
Adam Turnbull, . — Nervorum Connexu.
Alex. Binny Webster — Aneurysmate.
George Wilkie, . — Rheumatismo.
Thomas A. Wise, . — Topographia Medica.
Cornelius Wood . — Erysipelate Endemico.
David Woodburn, . — Febre Hectica.
Alexander C. Young, — Epilepsia.

Of England.

Gervase Alexander, — Apoplexia.
J. Mainwaring Brander, De Sanguinis Distributione.
Robert Chaffey, . — Opio.
Wm. Ross Chapman, — Hydrothorace
John Rowe Croudace, — Enteritide.
Thomas Dowler, . — Rheumatismo Acuto.
Henry Gaulter, . — Insania.
Thomas Green, . — Dysenteria.
William Haslewood, — Apoplexia Sanguinea.
Thomas Hingston, — Morbo Comitiali.
Charles Holland, . — Absorptione.
Francis Hutchinson, — Animalium Tepore.
John B. James, . — Vi Noctis in Corpus Humanum.
G. Deare Meadows, — Hydrothorace
John O'Donnell, . — Hernia.
John Overend, - — Morbis quibus Homines vel literis vel negotiis nimium dediti frequenter plectuntur.
Charles Philips, . — Scirrho Ventriculi.
Alex. John Ralph, . — Febre Barbadensi Legionem Reginæ II. 1816, 1817, populante.
Edmund Rendle, . — Vitalitate Sanguinis.
T. Welbank Robinson, — Acido Muriatico et Chlorino.
John Savery, . — Febre.
Geo. Augustus Starling, — Dyspepsia.
Geo. Fordyce Story, — Hydrocephalo Acuto.
Robert J. N. Streeten, — Delirio Trementi.
John Thompson, . — Febre Typho.
Charles Timm, . — Ligatura Arteriarum.
Chas. J. B. Williams, — Sanguine ejusque Mutationibus.
J. Calthrop Williams, — Stethoscopio.
Oswald C. Wood, . — Modo quo Nervi Motui inserviunt.
Robert Young, . — Hæmoptysi.

From Berwick.

William Dunbar How, — Epilepsia.

From Wales.

Godfrey Howitt, . — Calculis Urinariis.

From Ireland.

P. Ulic Burke, . De Hysteria.
John Copinger, . — Febre Puerperarum.
Bernard Coyne, . — Typho Graviore.
J. Mackay Cunningham, — Causis Morborum Excitantibus.
Charles Curtin, . — Dyspepsia.
Patrick Daly, . — Hernia.
Gilbert Elliot, . — Hepatitide.
Patrick Fitzpatrick, — Febribus Continuis generatim, sed præcipue de Typo.
Robert Foley, . — Ophthalmia.
Henry Fulton, . — Dysenteria.
Harpur Gamble, . — Commodis Auditus in Moribis Pectoris distinguendis.
William John Geary, — Usu Purgantium Mercurialium ampliori in Morbis quibusdam sub nomine Febrium cognitis.
Thomas Godfrey, . — Hæmorrhagia Uterina.
William M. Hawthorne,— Sanguinis Missione in Febribus.
George Howse, . . — Apoplexia.
Patrick Keatinge, . . — Phthisi Pulmonali.
John Edward Kidley, — Pneumonia.
T. A. Ahmuty Kirkwood,— Ascite.
Henry M'Cormac, — Clavo Secalino.
Samuel M'Mullan, . — Enteritide.
Robert And. M'Munn, — Abortu.
Adam Murray, .. — Ophthalmia.
John Murphy, . De Hydrocephalo Acuto.
John O'Reilly, . — Hemicrania.
Wil. Aug. O'Shea, . — Rheumatismo.
Robert Pack, . — Rheumatismo.
Peter Sherlock, . — Erysipelate.
Stephenson Teevan. . — Febre Continua.
Richard Townsend, . — Usu medico Sulphatis Quinæ.
Thomas Williams, . — Rubeola.

From Guernsey.

Peter Frederic De Jersey, — Vitiis Humani Cordis Congenitis.

From the East Indies.

Henry Dowdeswell, — Delirio Ebriositate Exorto.
Cuthbert Finch, . — Pulsu.
Henry Harington, — Mænorrhagia.
John Brathwait Taylor, — Absorptione per Venas.

From Jamaica.

Richard Bainbridge, . — Rebus Obstetricis.
James Lee Brodbelt, — Stricturis Recti.
Thomas Lee Brodbelt, — Apoplexiæ Varietatibus Entonica atque Atonica.
John M'Robbie, . . — Hydrocephalo
Will. Bromley Rose, . — Dysenteria.

From Demerara.

Thomas Cuming, . . — Phthisi Pulmonali.

From Nova Scotia.

John M'Braire, . — Febre Intermittente.

From America.

Francis Boott, . — Hydrocephalo.

The Medical Lectures of the University of Edinburgh for the ensuing Session, commence on Wednesday the 28th of October.

Dietetics, Materia Medica and Pharmacy, by Dr Duncan, *Junior*, at 8, *a. m.*
Practice of Medicine, Dr Home, 9, *a. m.*
Chemistry and Chemical Pharmacy, Dr Hope, 10, *a. m.*
Institutes of Medicine, Dr Duncan, *Senior*, and Dr Alison, 11, *a. m.*
Hospital, 12, *noon.*
Anatomy and Pathology, Dr Monro, 1, *p. m.*
Military Surgery, Dr Ballingall, 2, *p. m.*
Midwifery and Diseases of Women and Children, Dr Hamilton, 3, *p. m.*
Principles and Practice of Surgery, Dr Monro, 4, *p. m.*
Clinical Medicine, Dr Home and Dr Duncan, junior, 4, *p. m.*
Clinical Surgery, Mr Russell and Mr Alison, 5, *p. m.*

During the Summer Session, Lectures will be given on the following Branches of Medical Education:—

Botany, Dr Graham.
Midwifery, Dr Hamilton.
Clinical Medicine, Dr Home.
Clinical Surgery, Mr Russel.
Medical Jurisprudence, Dr Christison.

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